PROCEEDINGS

OF THE

ROYAL SOCIETY OF LONDON.

From June 2, 1892, *to February* 9, 1893.

VOL. LII.

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June 16, 1892.

No. 316.

PROCEEDINGS

OF

THE ROYAL SOCIETY.

June 2, 1892.

The Annual Meeting for the Election of Fellows was held this day

The LORD KELVIN, D.C.L., LL.D., President, in the Chair.

The Statutes relating to the election of Fellows having been read, General Clerk and Mr. Crookes were, with the consent of the Society, nominated Scrutators to assist the Secretaries in examining the lists.

The votes of the Fellows present were then collected, and the following candidates were declared duly elected into the Society :—

Armstrong, Robert Young, Lieut.-Col. R.E.
Beddard, Frank Evers, M.A.
Fleming, Professor John Ambrose, D.Sc.
Foster, Professor Clement Le Neve, D.Sc.
Gadow, Hans, M.A., Ph.D.
Giffen, Robert, LL.D.
Gotch, Professor Francis, M.A., M.R.C.S.
Herdman, Professor William Abbott, D.Sc.
Hutton, Capt. Frederick Wollaston.
Joly, John, M.A.
Larmor, Joseph, D.Sc.
Miall, Professor Louis C.
Peach, Benjamin Neve, F.R.S.E.
Pedler, Professor Alexander, F.I.C.
Waller, Augustus D., M.D.

Thanks were given to the Scrutators.

June 2, 1892.

The LORD KELVIN, D.C.L., LL.D., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "On the Method of Examination of Photographic Objectives at the Kew Observatory." By Major L. DARWIN. Communicated by Captain ABNEY, R.E., F.R.S. Received April 13, 1892.

(Abstract.)

The paper describes the method of examination of photographic objectives which has been adopted at the Kew Observatory, chiefly on the recommendation of the author. In selecting and devising the different tests, Major Darwin acted in co-operation with Mr. Whipple, the Superintendent of the Observatory, and was aided by consultations with Captain Abney.

The object of the examination is to enable any one, on the payment of a small fee, to obtain an authoritative statement or certificate as to the quality of an objective for ordinary purposes; the amount of time that can be devoted to each experiment is therefore strictly limited.

An example is first given of a "Certificate of Examination" such as would be obtained from Kew, and then the different tests are discussed in detail. Many of them are done by well-known methods, which need not here be mentioned. For the greater part of the examination, an apparatus called the "testing camera" is employed, and this is fully described in the paper. The following are the different items in the Certificate of Examination, including the various tests to which the objective is subjected, or the subjects about which information is given:—

(1.) to (4.) Under these headings are given a description of the objective, the date of receipt, the size of the plate for which the objective is to be examined, &c., &c. None of this information forms part of the result of the testing.

(5.) *Number of Reflecting Surfaces of the Lenses.*

(6.) *Centering in Mount.*

(7.) *Visible Defects, such as Veins, Feathers, &c.*

(8.) *Flare Spot.*

(9.) *Effective Aperture of Stops*, which is given for each one supplied with the objective. In recording the results, it is proposed that the system of numbering recommended by the International Photographic Congress of Paris of 1889 should be adopted.

(10.) *Angle of Cone of Illumination, &c.*—Under this heading several useful items of information are given, such as the extreme angle of the field which is illuminated by the objective without reference to definition; the angle of field which is required to cover the plate of the size for which the objective is being examined; and, the largest stop of which the whole opening can be seen from the

whole of the plate. With reference to this last item, it is shown how if a larger stop than the one here named is used, the illumination will fall off very rapidly towards the edges of the plate, whereas there will be no improvement in this respect by using a smaller stop.

(11.) *Principal Focal Length.*—This is found by revolving the camera through a known angle, and measuring the movement of the image of a distant object on the ground glass; with the testing camera it is so arranged that an angular movement can be given with great ease and accuracy, and that the angle is such that half the focal length is directly read off on a scale on the ground glass. The observation is made, and the focus adjusted, when the image is at a point some 14 degrees from the axis of the objective, and the effect of distortion and curvature of the field is discussed; it is proved that the focal length thus obtained, even though it may not be identical with the principal focal length as measured on the axis, is nevertheless what the photographer in reality wants to ascertain.

(12.) *Curvature of the Field.*—The amount of movement of the ground glass to adjust the focus at different parts of the field is measured, and the results are recorded to show the curvature. A table is given by which it can be seen what is the largest stop which can be used so as to produce negatives up to a certain standard of excellence in definition.

(13.) *Distortion.*—This test depends in principle on ascertaining the sagitta or deflection in the image of a straight line along one side of the plate. In the discussion it is shown that to give the total distortion near the edge of the plate would not answer practical requirements, and that the proposed method of examination does give the most useful information that can be supplied.

(14.) *Definition.*—This is found by ascertaining what is the thinnest black line the image of which is just visible when seen against a bright back-ground. It is shown that this is the best method that could be devised of measuring the defining power of an objective, and that it is not open to serious objections on theoretical grounds.

(15.) *Achromatism.*—Under this heading is recorded the difference of focus between an object when seen in white light and the same when seen in blue or red light. How to estimate approximately the diffusion in the image due to any defect in achromatism is discussed.

(16.) *Astigmatism.*—This test is performed by measuring the distance between the focal lines at the corner of the plate, and by calculating from the result thus obtained the approximate diameter of the disc of diffusion due to astigmatism. The reflection of a lamp in a thermometer bulb is employed to give a fine point of light, and the positions of the focal lines are obtained by noting when the image of the object appears as a fine line, first in one direction, and then in another, as the focus is altered.

(17.) *Illumination of the Field.*—The method of examination, which is due to Captain Abney, is described. The question of the falling off of the intensity of illumination from the centre of the plate is discussed.

II. "Supplementary Report on Explorations of Erect Trees containing Animal Remains in the Coal-Formation of Nova Scotia." By Sir J. William Dawson, F.R.S. Received April 25, 1892.

To the memoir which I had the honour to present to the Royal Society on this subject in 1882* I appended a note from Dr. Scudder, of Cambridge, U.S., so well known for his researches in fossil Insects and Arachnidans, in which he gave a preliminary account of the remains of Arthropods in my collections which I had submitted to him. He has only in the present year completed his examination of these remains, most of which are very fragmentary, and much damaged by unequal pressure. The result has been embodied in a Report on Canadian Fossil Insects, now in course of publication by the Geological Survey of Canada.

In this report he will describe from the contents of the Sigillarian stumps extracted by me, with the aid of the grant of this Society, three new species of Myriapoda, making, with the five previously known from these remarkable repositories, eight in all, belonging to two families, Archiulidæ and Euphoberidæ, and to three genera, *Archiulus*, *Xylobius*, and *Amynilyspes*. The three new species are *Archiulus euphoberioides*, Sc., *A. Lyelli*, Sc., and *Amynilyspes* (sp.). The remains of Scorpions he refers to three species, *Mazonia acadica*, Sc., *Mazonia* (sp.), and a third represented only by small fragments. The characters of the species referred to *Mazonia* he considers as tending to establish the generic distinctness of *Mazonia* from *Eoscorpius*. Dr. Scudder also notices the fragment of an insect's head containing part of a facetted eye, mentioned in my memoir, and considers it probably a portion of a Cockroach.

Much credit is due to Dr. Scudder for the care and skill with which he has worked up the mostly small and obscure fragments which I was able to submit to him, and which are probably little more than *débris* of the food of the Amphibians living for a time in these hollow stumps, and devouring such smaller animals as were so unfortunate as to be imprisoned with them. In this connexion the suggestion of Dr. Scudder is worthy of attention, that the scaly armour of the smaller Microsaurians may have been intended to defend them against the active and venomous Scorpions which were their contemporaries, and some of which were sufficiently large to

* 'Phil. Trans.,' 1882, p. 621.

be formidable antagonists to the smaller land Vertebrates of the period.

The report of Dr. Scudder will complete the account of the land animals of the erect Sigillariæ of the South Joggins, unless by new falls of the cliff fresh trees should be exposed. From 1851, when the first remains were obtained from these singular repositories by the late Sir Charles Lyell and the writer, up to the present time, they have afforded the remains of twelve species of Amphibians, three land Snails, eight Millipedes, three Scorpions, and an Insect.

The type specimens of these animals have been placed in the Peter Redpath Museum of McGill University, and such duplicates as are available will be sent to the British Museum and that of the Geological Survey of Canada.

III. "The Hippocampus." By Alex Hill, M.D., Master of Downing College. Communicated by Prof. A. Macalister, M.D., F.R.S. Received May 4, 1892.

(Abstract.)

The subject of this paper is the hippocampal region of the brain in anosmatic animals. Several specimens of the brain of the bottle-nosed whale, narwhal, porpoise, and calf-seal were obtained for the purpose of studying the extent to which the hippocampus in animals totally destitute of the sense of smell, or possessing it in a very small degree, departs from the ordinary type.

The hippocampal region was, in each case, cut into a series of ections, which showed that there is, in the brains of *Hyperoodon* and *Monodon*, no fascia dentata; in *Phocæna* this formation is very rudimentary; in *Phoca* it is rather less strongly developed than in man.

As a standard of comparison, the hippocampus of the ox was studied in a similar manner. Certain points which have not been described hitherto with regard to the anatomy and histology of the hippocampus in macrosmatic brains are noticed. Other peculiarities in the general form and in minute structure which distinguish the brains of anosmatic animals are also mentioned incidentally.

The discovery that the fascia dentata is completely absent in animals which possess no olfactory bulb or tract, and that it varies in development directly as the size of these organs, throws light upon the function of the hippocampal region, and seems to call for a fresh definition of its several parts and a revision of the nomenclature of the region. The paper contains an historical survey of the terminology and suggestions for its revision.

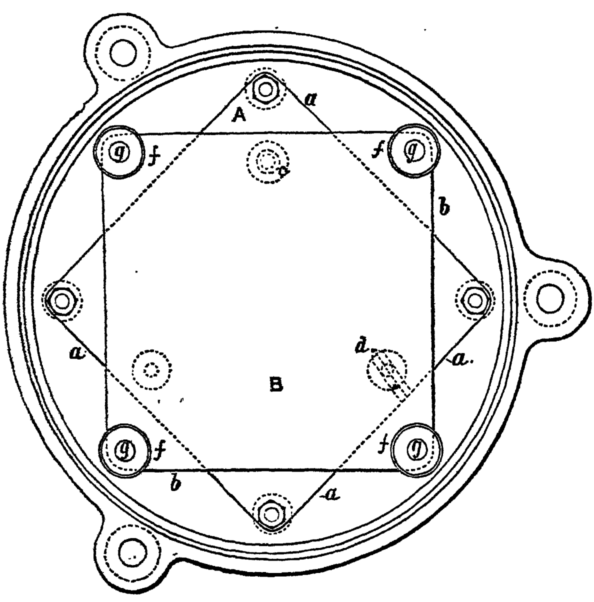
IV. "On a new Form of Air Leyden, with Application to the Measurement of Small Electrostatic Capacities." By LORD KELVIN, P.R.S. Received May 31, 1892.

In the title of this paper as originally offered for communication "*Air Condenser*" stood in place of "*Air Leyden,*" but it was accompanied by a request to the Secretaries to help me to a better designation than "Air Condenser" (with its ambiguous suggestion of an apparatus for condensing air), and I was happily answered by Lord Rayleigh with a proposal to use the word "leyden" to denote a generalised Leyden jar, which I have gladly adopted.

The apparatus to be described affords, in conjunction with a suitable electrometer, a convenient means of quickly measuring small electrostatic capacities, such as those of short lengths of cable.

The instrument is formed by two mutually insulated metallic pieces, which we shall call A and B, constituting the two systems of an air condenser, or, as we shall now call it, an air leyden. The systems are composed of parallel plates, each set bound together by four long metal bolts. The two extreme plates of set A are circles of much thicker metal than the rest, which are all squares of thin

FIG. 1.



sheet brass. The set B are all squares, the bottom one of which is of much thicker metal than the others, and the plates of this system are one less in number than the plates of system A. The four bolts binding together the plates of each system pass through well-fitted holes in the corners of the squares; and the distance from plate to plate of the same set is regulated by annular distance pieces which are carefully made to fit the bolt, and are made exactly the same in all respects. Each system is bound firmly together by screwing home nuts on the ends of the bolts, and thus the parallelism and rigidity of the entire set is secured.

The two systems are made up together, so that every plate of B is between two plates of A, and every plate of A, except the two end ones, which only present one face to those of the opposite set, is between two plates of B. When the instrument is set up for use, the system B rests by means of the well-known "hole, slot, and plane arrangement,"* engraved on the under side of its bottom plate, on three upwardly projecting glass columns which are attached to three metal screws working through the sole plate of system A. These screws can be raised or lowered at pleasure, and by means of a gauge the plates of system B can be adjusted to exactly midway between, and parallel to, the plates of system A. The complete leyden stands upon three vulcanite feet attached to the lower side of the sole plate of system A.

In order that the instrument may not be injured in carriage, an arrangement, described as follows, is provided by which system B can be lifted from off the three glass columns and firmly clamped to the top and bottom plates of system A.

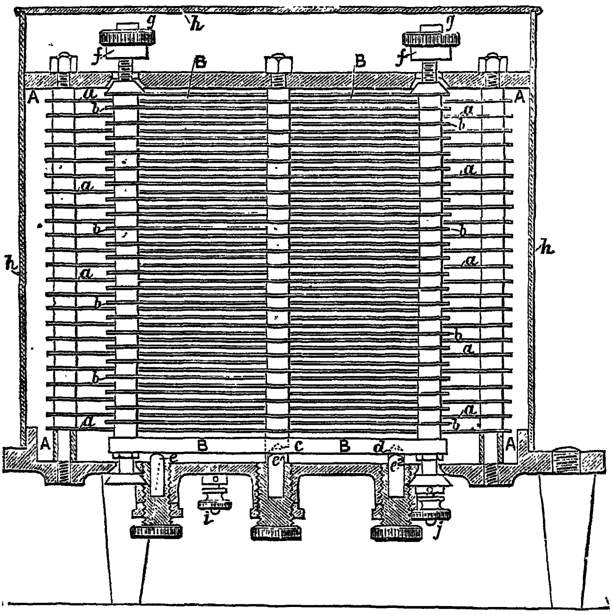
The bolts fixing the corners of the plates of system B are made long enough to pass through wide conical holes cut in the top and bottom plates of system A, and the nuts at the top end of the bolts are also conical in form, while conical nuts are also fixed to their lower ends below the base plate of system A. Thumbscrew nuts, *f*, are placed upon the upper ends of the bolts after they pass through the holes in the top plate of system A.

When the instrument is set up ready for use these thumbscrews are turned up against fixed stops, *g*, so as to be well clear of the top plate of system A; but when the instrument is packed for carriage they are screwed down against the plate until the conical nuts mentioned above are drawn up into the conical holes in the top and bottom plates of system A; system B is thus raised off the glass pillars, and the two systems are securely locked together so as to prevent damage to the instrument.

A dust-tight cylindrical metal case, *h*, which can be easily taken off for inspection, covers the two systems and fits on to a flange on

* Thomson and Tait's 'Natural Philosophy,' § 198, example 3.

FIG. 2.



system A. The whole instrument, as said above, rests on three vulcanite legs attached to the base plate of system A; and two terminals are provided, one, *i*, on the base of system A, and the other, *j*, on the end of one of the corner bolts of system B.

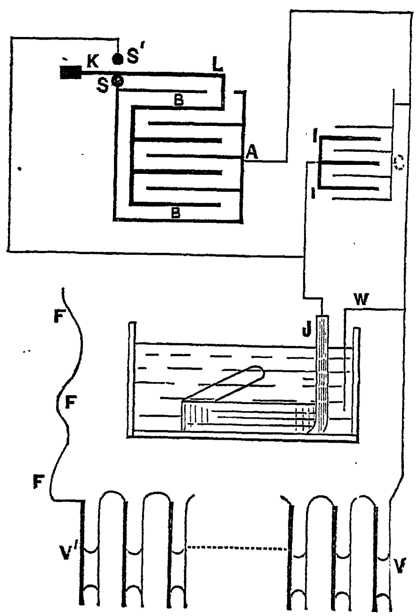
The air leyden which has been thus described is used as a standard of electrostatic capacity. In the instrument actually exhibited to the Society there are twenty-two plates of the system B, twenty-three of the system A, and therefore forty-four octagonal air spaces between the two sets of plates. The thickness of each of these air spaces is approximately 0·301 of a centimetre. The side of each square is 10·13 cm., and therefore the area of each octagonal air space is 85·1 sq. cm. The capacity of the whole leyden is therefore approximately $44 \times 85{\cdot}1/(4\pi \times 0{\cdot}301)$, or 990 cm. in electrostatic measure; or $1{\cdot}1 \times 10^{-18}$ c.g.s., electromagnetic measure; or $1{\cdot}1 \times 10^{-9}$ farads, or $1{\cdot}1 \times 10^{-3}$ microfarads. This is only an approximate estimate founded on a not minutely accurate measurement of dimensions, and not corrected for the addition of capacity, due to the edges and projecting angles of the squares and the metal cover. I hope to have the capacity determined with great accuracy by comparison with Mr. Glazebrook's standards in Cambridge.

To explain its use in connexion with an idiostatic electrometer for the direct measurement of the capacity of any insulated conductor, I shall suppose, for example, this insulated conductor to be the

insulated wire of a short length of submarine cable core, or of telephone, or telegraph, or electric light cable, sunk under water, except a projecting portion to allow external connexion to be made with the insulated wire.

The electrometer which I find most convenient is my "multicellular voltmeter," rendered practically dead-beat by a disc under oil hung on the lower end of the long stem carrying the electric "needles" (or movable plates). In the multicellular voltmeter used in the experimental illustration before the Royal Society, the index shows its readings on a vertical cylindric surface, which for electric light stations is more convenient than the horizontal scale of the multicellular voltmeters hitherto in use; but for the measurement of electrostatic capacity the older horizontal scale instrument is as convenient as the new form.

FIG. 3.



To give a convenient primary electrification for the measurement, a voltaic battery, VV' (fig. 3), of about 150 or 200 elements, of each of which the liquid is a drop of water held up by capillary attraction between a zinc and copper plate about 1 mm. asunder. An ordinary electric machine, or even a stick of rubbed sealing-wax may, however, be used, but not with the same facility for giving the amount of electrification desired as the voltaic battery.

One end of the voltaic battery is kept joined metallically to a wire,

W, dipping in the water in which the cable is submerged, and with the case C of the multicellular, and with the case and plates A of the leyden, and with a fixed stud, S, forming part of the operating key to be described later. The other end of the voltaic battery is connected to a flexible insulated wire, FFF, used for giving the primary electrification to the insulated wire J of the cable, and the insulated cells II of the multicellular kept metallically connected with J. The insulated plates, B, of the leyden are connected to a spring, KL, of the operating key referred to above, which, when left to itself, presses down on the metal stud S, and which is very perfectly insulated when lifted from contact with S by a finger applied to the insulating handle K. A second well insulated stud, S′, is kept in metallic connection with J and I (the insulated wire of the cable and the insulated cells of the multicellular).

To make a measurement the flexible wire F is brought by hand to touch momentarily on a wire connected with the stud S′, and immediately after that a reading of the electrometer is taken and watched for a minute or two to test either that there is no sensible loss by imperfect insulation of the cable and the insulated cells of the multicellular, or that the loss is not sufficiently rapid to vitiate the measurement. When the operator is satisfied with this he records his reading of the electrometer, presses up the handle K of the key, and so disconnects the plates B of the leyden from S and A, and connects them with S′, J, I. Fifteen or twenty seconds of time suffices to take the thus diminished reading of the multicellular, and the measurement is complete.

The capacity of the cable is then found by the analogy:—As the excess of the first reading of the electrometer above the second is to the second, so is the capacity of the leyden to the capacity of the cable.

The preceding statement describes the arrangement which is most convenient when the capacity of the cable exceeds the capacity of the leyden. The plan which is most convenient in the other case, that is to say, when the capacity of the cable is less than that of the leyden, is had by interchanging B and J throughout the description. In this case, a charge given to the leyden is divided between it and the cable. The capacity of the cable is then found by the analogy:—As the second reading of the electrometer is to the excess of the first above the second, so is the capacity of the leyden to the capacity of the cable.

A small correction is readily made with sufficient accuracy, for the varying capacity of the electrometer, according to the different positions of the movable plates, corresponding to the different readings, by aid of a table of corrections determined by special measurements for capacity of the multicellular.

V. "On Certain Ternary Alloys. Part VI. Alloys containing Aluminium, together with Lead (or Bismuth) and Tin (or Silver)." By C. R. ALDER WRIGHT, D.Sc., F.R.S., Lecturer on Chemistry and Physics in St. Mary's Hospital Medical School. Received May 13, 1892.

The experiments described in the previous five papers* have shown *inter alia* that when the critical curves are delineated deduced with lead (or bismuth) as heavier immiscible metal, zinc as lighter ditto, and tin (or silver) as "solvent," the curves where bismuth is substituted for lead lie *inside*, and those where silver is substituted for tin *outside*, the original curves; and, further, that in certain cases the formation of definite atomic compounds between particular pairs of metals leads to marked results as regards the mutual relations of the various pairs of conjugate points. Thus, silver and zinc form the compounds $AgZn_5$ and Ag_4Zn_5, with the result of producing marked bulging (inwards or outwards) of the curves deduced with these two metals associated with either lead or bismuth. Tin and zinc appear to form the compound $SnZn_4$, with the result of causing a peculiar convergence of the tie-lines when the proportions of metals mixed together permit of the formation of lighter alloys containing tin and zinc in approximately this ratio. Again, the alloys of lead, zinc, and tin are remarkable in that the lower ties slope in one direction, and the upper ones in the opposite direction; and the position where the angle of slope of the lower ties is greatest (which is approximately the point where the excess of tin percentage in the lighter alloy over that in the heavier one is a maximum) is situated very near to that corresponding with a ratio of tin to lead in the heavier alloy denoted by the formula $SnPb_3$.

The following experiments were made with mixtures of metals corresponding with those previously described, excepting that *aluminium* was substituted for *zinc*. One general result appears to be that this substitution always causes the curve to lie *outside* of its former position; and another, that as aluminium does not appear to unite with silver to form compounds analogous to $AgZn_5$ and Ag_4Zn_5, the peculiar bulges observed with silver-zinc-lead and silver-zinc-bismuth alloys *are not visible* with silver-aluminium-lead and silver-aluminium-bismuth alloys.

These experiments were commenced about four years ago, at which time moderately pure aluminium was not in the market in any quantity; at least, a number of samples of metals prepared by

* Part I, 'Roy. Soc. Proc.,' vol. 45, p. 461: Part II, vol. 48, p. 25; Part III, vol. 49, p. 156; Part IV, vol. 49, p. 174; Part V, vol. 50, p. 372.

different makers were examined with the object of obtaining a few pounds of aluminium in the highest practicable state of purity; but, although several of these were stated to contain 98 to 99 per cent. of aluminium and upwards, on analysis far greater amounts of impurity than 1 or 2 per cent. were generally found to be present. Thus, one sample of the so-called pure metal contained upwards of 10 per cent. of iron; another contained, besides some iron, 2·05 per cent. of matters other than silicon and silica insoluble in aqua regia, and 2·91 per cent. of silicon, partly oxidised by aqua regia to soluble silicic acid, partly oxidised but not dissolved, and partly not oxidised at all. The batch finally selected as the best obtainable had the following composition, the sample being drawn from a considerable mass melted together; it was supplied as metal of as nearly perfect purity as commercially possible to obtain, and containing upwards of 99 per cent. of aluminium:—

Silicon left undissolved and unoxidised by aqua regia	0·30	
Silicon oxidised but not dissolved by aqua regia	1·01	
„ „ and dissolved by aqua regia	0·13	
Total silicon		1·44
Iron		2·75
Aluminium (by difference)		95·81
		100·00

No other impurities of any kind could be detected.*

When this metal was employed in the production of a compound ternary ingot it was found that almost the whole of the silicon and iron were contained in the lighter alloy in which the aluminium predominated (as compared with the heavier alloy). In some cases, however, when the amount of aluminium in the heavier alloy became

* [At the present time aluminium containing notably smaller quantities of silicon and iron can be obtained commercially at less than one-eighth the price charged in 1888. Thus a recent sample of ordinary deliveries of "Neuhausen" aluminium was found to contain—

Silicon left undissolved and unoxidised by aqua regia	0·41	
Silicon oxidised but not dissolved	0·07	
„ „ and dissolved by aqua regia	0·83	
Total silicon		1·31
Iron		0·39
Copper		0·07
Aluminium (by difference)		98·23
		100·00

May 30, 1892.]

considerable, small amounts of iron and silicon were also contained therein.

In some cases, when the amount of aluminium in the upper alloy was large and the other constituents small, the aluminium was taken by difference, the percentages being corrected by means of the above analysis to what they would have been if reckoned on the sum of the aluminium and the other two metals as 100. When, however, the other constituents were present in larger quantity the aluminium was directly determined, the alumina ultimately precipitated being collected and weighed, and the SiO_2 and Fe_2O_3 present therein subsequently determined and subtracted so as to obtain a corrected determination of the aluminium; the percentages were then reckoned on the sum of the aluminium thus found and the other two metals as 100. Thus, for example, an alloy of tin, lead, and aluminium was found to contain 12·01 per cent. of tin and 1·71 per cent. of lead; hence, reckoning the difference, 86·28 per cent., as aluminium, silicon, and iron containing 95·81 per cent. of the first, the corrected analysis is—

Tin........................	12·01	=	12·46
Lead......................	1·71	=	1·77
Aluminium...86·28 × 0·9581 =	82·66	=	85·77
	96·38		100·00

On the other hand, an alloy of silver, aluminium, and lead was found to contain the following percentages of these three metals respectively, the aluminium being reckoned from the weight of alumina after subtraction of silica and ferric oxide contained in the mixed precipitate first weighed; whence the annexed composition, calculated on the sum of the three metals as 100:—

	Found.	Calculated on sum as 100.
Silver..............	80·17	80·79
Lead	6·55	6·60
Aluminium.........	12·51	12·61
	99·23	100·00

In the case of the lighter alloys containing tin, aluminium, and lead (or bismuth) with large percentages of the first metal, it was sometimes found that a notable amount of oxygen was absorbed by the ingot whilst standing molten for eight hours, so that a perceptible deficiency from 100 was observed when all the constituents were added together. With silver-aluminium-lead and silver-aluminium-bismuth alloys this was not the case; on the other hand,

alloys containing antimony instead of tin or silver absorbed oxygen still more readily. In all such cases the analyses are calculated upon the sum of the aluminium, tin, and lead as 100; thus in the following instance:—

	Directly determined.	Calculated on sum as 100.
Tin................	52·53	54·82
Lead...............	8·16	8·52
Aluminium.........	35·12	36·66
Iron................	0·99	—
Silicon (total).......	0·51	—
Oxygen (by difference)	2·69	—
	100·00	100·00

Mixtures of Aluminium, Lead, and Tin.

A number of ternary mixtures were prepared by melting aluminium, and then adding weighed quantities of lead and tin, and stirring vigorously. A considerable amount of scoriæ was usually formed, the lead partially oxidising, and the presence of the lead oxide formed causing the mass to "flour" considerably; usually the molten portion was run off into an ingot mould, and then re-melted, and when in the pasty stage preceding complete fusion rubbed about in the bottom of the crucible with a hot fireclay pestle, so as to promote intermixture. Finally the temperature was raised, so as to bring about complete fusion, and after more vigorous stirring the liquid metal was poured into red-hot narrow clay crucibles or test-tubes, and maintained molten in a lead bath for seven to eight hours in the manner previously described. The temperature during this period of tranquil fusion lay between 750° and 850°, and averaged close upon 800°, as determined by the platinum specific heat pyrometer (Part I). No flux of any kind was used during the previous meltings and stirrings, oxidation being diminished as far as practicable by directing a current of coal gas into the crucible; notwithstanding a much larger amount of oxidation usually took place than with metallic mixtures fused together with potassium cyanide, as in the previous experiments. The metal thus oxidised was usually chiefly *lead*, the tin and aluminium present being comparatively unaffected; the uncertainty as to the amount of lead thus removed in any given case prevented any accurate calculation being made respecting the relative weights of the three metals present in the final compound ingot ultimately obtained; the weight of this was always considerably less than the joint weights of the three metals originally used, partly through oxidation, but principally because the formation of oxide led to a copious amount of

"flouring" of the molten metal, a considerable proportion of the alloys being reduced to minute particles that did not coalesce with the rest of the molten mass, and remained in the crucible when the fused metal was poured into the clay test-tubes. This same flouring action was equally observed when bismuth was substituted for lead; whilst with silver as solvent metal instead of tin it was still more marked; frequently not more than three-quarters or two-thirds of the metals originally weighed up (40 to 50 grammes) were obtained as final compound ingot, and sometimes even less; the deficiency varying according to the success or otherwise of attempts to exclude access of air whilst stirring.

The analysis of the alloys thus prepared was carried out as follows:—A weighed portion was dissolved in hydrochloric acid containing a little nitric acid, and the solution largely diluted. Sulphuretted hydrogen in excess was then passed through, and the solution containing aluminium, &c., filtered off from the precipitated sulphides of tin and lead. These were then separated by sulphide of ammonium, and finally weighed as SnO_2 and $PbSO_4$ in the usual way. The filtrate was saturated with sulphuretted hydrogen, and allowed to stand two or three days to ensure the separation of the last traces of tin; finally ammonia was added, and the precipitate collected, washed, and ignited. The weighed impure alumina was then redissolved by long boiling with hydrochloric acid, any silicic acid present separated by evaporation to dryness, &c., and the iron present estimated volumetrically. It was found impracticable to obtain good results by treating the alloys with nitric acid alone, not only because alloys containing much aluminium are only attacked by that acid with great difficulty, but also because the tin as determined by weighing the undissolved SnO_2 was over-estimated to an unknown amount on account of the presence of silicon, partly left undissolved as such, partly converted into silicic acid more or less retained by the tin oxide; so that it was necesary to fuse the impure SnO_2 with sulphur and sodium carbonate to convert into soluble sulphostannate, to precipitate tin sulphide from the filtered aqueous solution by acidulation, and ultimately to roast and weigh as SnO_2.*

The following corrected percentages (reckoned on tin + lead + aluminium = 100) were obtained from the examination of 25 compound ingots representing 50 alloys. In the preparation of these the lead and aluminium were originally weighed up in the proportion of about 2·5 to 1 for the ingots containing smaller amounts of tin; on account of the low density of aluminium this resulted in the formation of approximately equal *volumes* of heavier and lighter alloy

* With aluminium-bismuth-tin alloys the SnO_2 undissolved by nitric acid also retains bismuth left behind as sulphide on conversion into sulphostannate and treatment with water (*vide* Part III).

formed in each case. With the mixtures yielding the upper tie-lines, the proportions were altered successively to 1·5 to 1, and, finally, to 1 to 1, as the difference in density between the two alloys formed diminished; so as still to give rise to the formation of the two alloys in approximately equal quantities by volume. It was found that when lead and aluminium (without tin) were fused together, well intermixed, and then allowed to stand molten at about 800° for some hours, the lead retained, on an average, 0·07 per cent. of aluminium dissolved, and the aluminium 1·91 per cent. of lead (mean of nine experiments altogether).

No. of tie-line.	Heavier alloy.			Lighter alloy.			Excess of tin percentage in lighter alloy over that in heavier.
	Tin.	Lead.	Aluminium.	Tin.	Lead.	Aluminium.	
0	0	99·93	0·07	0	1·91	98·09	0
1	4·60	95·33	0·07	6·25	1·86	91·89	+ 1·65
2	11·17	88·77	0·06	14·50	2·33	83·17	+ 3·33
3	12·51	87·43	0·06	20·26	2·22	77·52	+ 7·75
4	14·01	85·75	0·24	24·50	2·55	72·95	+10·49
5	15·97	83·61	0·42	27·00	2·99	70·01	+11·03
6	18·25	81·42	0·33	27·47	2·89	69·64	+ 9·22
7	21·13	78·40	0·47	28·98	3·39	67·63	+ 7·85
8	25·10	74·17	0·73	31·83	3·38	64·79	+ 6·73
9	33·64	65·35	1·01	39·37	5·02	55·61	+ 5·73
10	45·41	51·89	2·70	45·34	5·58	49·08	− 0·07
11	55·16	39·73	5·11	48·11	5·78	46·11	− 7·05
12	61·52	29·14	9·34	52·27	7·16	40·57	− 9·25
13	64·21	24·52	11·27	54·62	9·38	36·00	− 9·59
14	65·07	23·06	11·87	59·14	15·00	25·86	− 5·93
15	63·92	21·78	14·30	60·27	17·04	22·69	− 3·65

The figures in the last column show that aluminium-tin-lead alloys resemble zinc-tin-alloys in that they furnish a critical curve, the lower ties of which slope to the left (lead side), and the upper ones to the right (zinc or aluminium side). Moreover, the point where the slope of the lower ties is greatest (*i.e.*, practically that where the excess of tin percentage in lighter alloy over that in heavier alloy is a + maximum) is in each case close to that where the ratio of tin and lead contained in the heavier alloy is that represented by the formula $SnPb_3$.

	Calculated for $SnPb_3$.	Al,Pb,Sn alloys (5th tie-line).		Found.	Zn,Pb,Sn alloys. Part V.
Tin......	15·97	15·97	=	16·04	15·41
Lead....	84·03	83·61	=	83·96	84·59
	100·00	99·58		100·00	100·00

This coincidence would seem to suggest that the peculiar disposition of tie-lines with these two series of alloys (not observed with any others) is due to the tendency towards the formation of the definite compound, $SnPb_3$; on the other hand, in neither case is any bulge inwards or outwards noticeable in the contour of the critical curve at the part corresponding with this compound, such as is observed in the case of the atomic compounds $AgZn_5$ and Ag_4Zn_5.

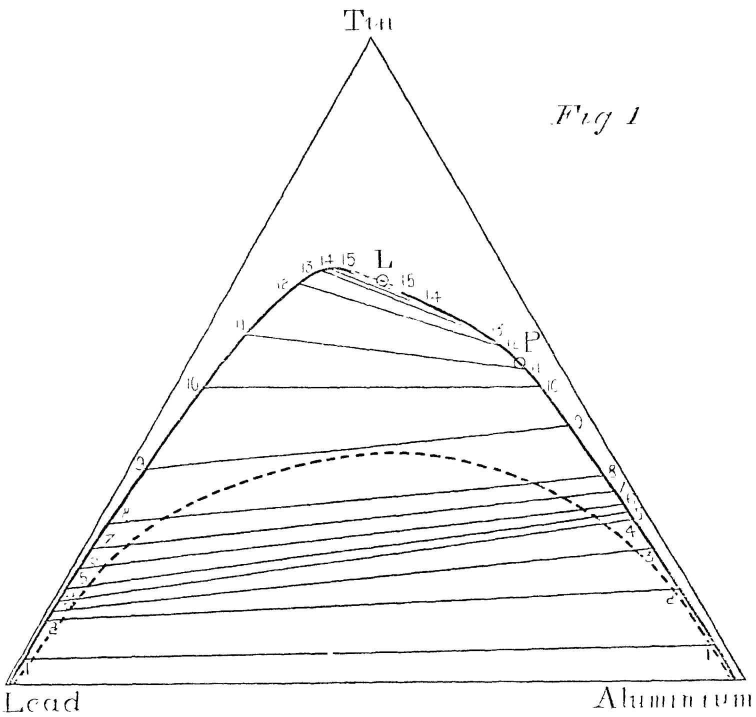


Fig. 1 represents the critical curve plotted from the figures contained in the above table; obviously it lies *outside* the analogous curves obtained at 650° and 800° with zinc-lead-tin alloys (Part V, figs. 4 and 5). The former of these is shown by the inner dotted line. The slopes of the upper ties suggest that there is some tendency towards convergence to a point on the right-hand side; the central point of that part of the curve where this tendency is manifest (marked P in the figure) is that representing the composition tin = 49, lead = 6, aluminium = 45, which is close to that represented by the formula $SnAl_4$, so far as the ratio between tin and aluminium is concerned.

	Calculated.	Found.		
Tin..........	52·2	49	=	52·1
Aluminium ...	47·8	45	=	47·9
	100·0	94		100·0

An analogous point, more clearly marked, was observed with zinc-lead-tin alloys corresponding with the formula $SnZn_4$ (Part V).

On applying the graphical methods of Sir G. G. Stokes for deducing the position of the limiting point L from the above data (Part V), the first method gives the values

$$A-B = 0·6$$
$$C+C' = 125·0$$

and the second—

$$A+A' = 39·5$$
$$B+B' = 35·0$$

from which the following percentages are deduced:—

	1st Method.	2nd Method.	Mean.
Lead..............	19·0	19·75	19·4
Aluminium........	18·5	17·5	18·0
Tin...............	62·5	62·75	62·6
	100·0	100·00	100·0

This mean value represents a ratio of lead to aluminium not far from that indicated by the formula $PbAl_7$.

	Calculated.	Found.		
Lead..............	52·3	19·4	=	51·9
Aluminium	47·7	18·0	=	48·1
	100·0	37·4		100·0

The corresponding ratio with lead-zinc-tin alloys was nearly that indicated by $PbZn_6$.

Mixtures of Aluminium, Bismuth, and Tin.

A series of mixtures of aluminium, bismuth, and tin was prepared in just the same way; the analysis of the ternary alloys formed was carried out in precisely the same fashion, excepting that the bismuth sulphide separated from tin sulphide was weighed ultimately as Bi_2O_3; the results calculated on the sum of aluminium + bismuth + tin = 100 are given in the table below, derived from the examination of 24 compound ingots, representing 48 alloys. As before, the

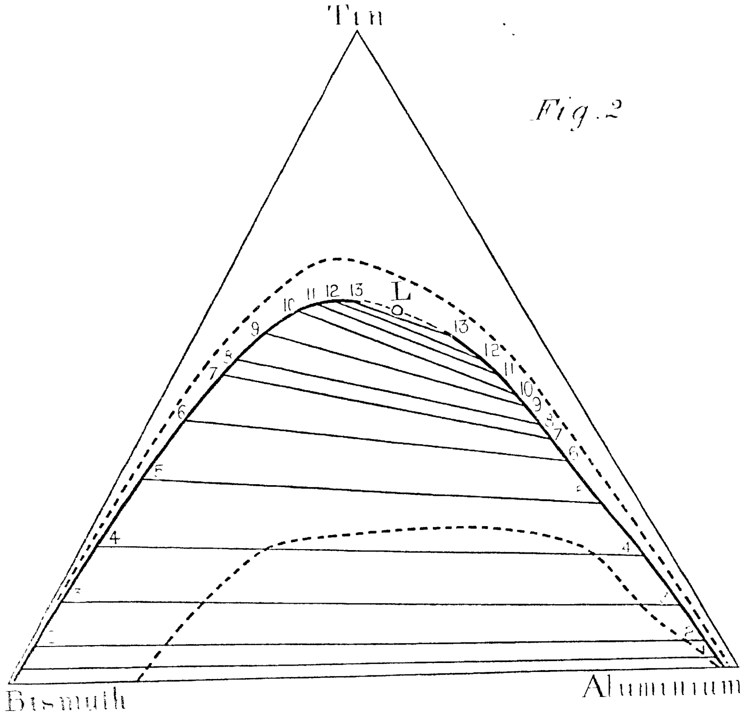
mixtures were kept molten for 7 to 8 hours at a temperature lying between 750° and 850°, and averaging near 800° C.; the earlier ties were obtained with mixtures containing bismuth and aluminium in the ratio by weight of 2·5 to 1, and the later ones in the ratio 1·5 to 1, and, finally, 1 to 1, in such fashion as to effect approximate equality in *volume* of the heavier and lighter alloys formed throughout.

No. of tie-line.	Heavier alloy.			Lighter alloy.			Excess of tin percentage in lighter alloy over that in heavier.
	Tin.	Bismuth.	Aluminium.	Tin.	Bismuth.	Aluminium.	
0	0	99·72	0·28	0	2·02	97·98	0
1	2·79	96·64	0·57	2·45	2·40	95·15	− 0·34
2	5·57	93·58	0·85	4·90	2·75	92·35	− 0·67
3	12·26	86·03	1·71	9·43	3·06	87·51	− 2·83
4	21·02	76·90	2·08	17·56	3·50	78·94	− 3·46
5	31·37	65·19	3·44	26·40	5·27	68·33	− 4·97
6	40·76	54·37	4·87	34·18	6·99	58·83	− 6·58
7	46·13	47·60	6·27	35·63	8·09	56·28	−10·50
8	50·09	41·64	8·27	38·12	8·37	53·51	−11·97
9	54·22	35·82	9·96	42·80	9·09	48·11	−11·42
10	56·91	30·73	12·36	42·97	9·49	47·54	−13·94
11	57·61	27·10	15·29	45·76	9·53	44·71	−11·85
12	58·17	24·01	17·82	49·48	13·08	37·44	− 8·69
13	57·23	23·20	19·57	51·32	14·37	34·31	− 5·91

Fig. 2 represents these values plotted on the triangular system, the outer dotted line being the curve above described for aluminium-lead-tin alloys, obviously lying *outside* the curve obtained with bismuth instead of lead. The inner dotted line represents the corresponding curve with zinc-bismuth-tin (Part V, fig. 7), illustrating the effect of substituting aluminium for zinc.* With the exception that the one lies outside the other, the aluminium-bismuth-tin and zinc-bismuth-tin curves exhibit a close resemblance, the ties uniformly sloping to the right (*i.e.*, the heavier alloy always containing more tin than the lighter one). No marked bulge inwards or outwards, indicating the formation of an atomic compound, is anywhere perceptible.

The position of the limiting point with zinc-bismuth-tin alloys at 650° was deduced (Part V) as yielding a ratio between bismuth and zinc close to that indicated by the formula $BiZn_{10}$; that similarly

* The solubility of bismuth in aluminium (without the presence of a third metal) is but little less than that of bismuth in zinc (2·02 per cent. at 870° as compared with 2·32 at 650° and 2·47 at 750°); but the solubility of aluminium in bismuth is greatly inferior to that of zinc in bismuth (0·28 per cent. at 870° as compared with 14·28 per cent. at 650° and 15·18 at 750°).



deduced with aluminium-bismuth-tin alloys corresponds with the formula $BiAl_{10}$. Applying Stokes' two methods, large-scale plottings lead to the values $A-B = -8{\cdot}2$ and $C+C' = 111{\cdot}0$ for the first method, and $A+A' = 38{\cdot}0$, $B+B' = 50{\cdot}0$ for the second method whence the limiting point, L, corresponds to the composition—

	1st Method.	2nd Method.	Mean.
Bismuth	18·2	19·0	18·60
Aluminium	26·3	25·0	25·65
Tin	55·5	56·0	55·75
	100·0	100·0	100·00

	Calculated for $BiAl_{10}$.	Found.		
Bismuth	43·3	18·6	=	42·1
Aluminium	56·7	25·65	=	57·9
	100·0	44·25		100·0

Mixtures of Aluminium, Lead, and Silver.

The foregoing experiments show that no material alteration is produced in the general characters of the critical curves for alloys containing lead (or bismuth) and zinc as immiscible metals and tin as solvent metal, when aluminium is substituted for zinc, excepting that the curves are raised through the diminished solubility of the two immiscible metals jointly in the solvent. The following experiments show that a similar result follows when silver is substituted for tin as solvent; with the difference that, whereas bulges exist in the lower parts of the critical curves when silver and zinc are simultaneously present, owing to the formation of the definite compounds $AgZn_5$ and Ag_4Zn_5 (Part V.), no such bulges are noticeable when silver and aluminium are employed; suggesting either that corresponding compounds of silver and aluminium are not formed at all; or that, if they are formed, they are more readily dissociated and broken up on standing molten for seven or eight hours than is the case with the silver-zinc compounds.

A series of mixtures of aluminium, silver, and lead was prepared, precisely as above described (the same aluminium being used), the only difference being that, on account of the higher fusing point of some of the alloys formed, the crucible in which the first fusion and intermixture took place was heated by a small Fletcher's blast gas furnace, instead of a large Bunsen gas burner, provided with a clay jacket and iron chimney, the heat from which, though sufficient for all the mixtures previously described, was not always high enough to permit of the less fusible highly argentiferous alloys being poured into the clay test tubes cleanly, and without partly solidifying in the crucible during the transference. The temperature of the lead bath was similarly increased by using differently arranged burners in such fashion as to maintain a perceptibly higher temperature than any hitherto used, ranging between 830° and upwards of 900°, and averaging about 870° or somewhat higher.

For the lower ties (smaller silver percentages) the lead and aluminium were used in the proportions 2 to 1, so that the volumes of the heavier and lighter alloys formed should not differ widely; for the upper ones increasing proportions of lead were found to be requisite to bring about the same result, so that for the highest ties obtained the lead and aluminium were originally used in the proportion 10 to 1. Owing to oxidation of lead, however, the ratio of the quantities of the two metals present in the compound ingots finally obtained was perceptibly less than this, by an amount too variable to allow an average to be even approximately estimated.

The analysis was made by dissolving in nitric acid and precipitating silver as chloride; the filtrate was evaporated with excess of sulph-

uric 'acid, and the lead determined as $PbSO_4$; the filtrate from this was supersaturated with ammonia, and the weight of the impure alumina found corrected for silica and ferric oxide as before, the results being finally calculated on the sum of silver + aluminium + lead as 100.

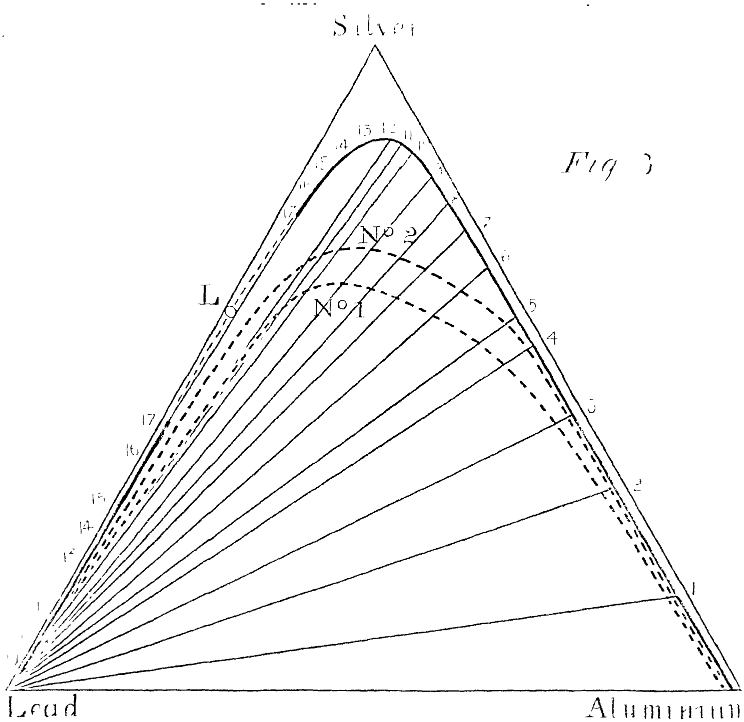
The following table represents the results obtained by the examination of 26 compound ingots forming 52 ternary alloys.

No. of tie-line.	Heavier alloy.			Lighter alloy.			Excess of silver percentage in lighter alloy over that in heavier.
	Silver.	Lead.	Aluminium.	Silver.	Lead.	Aluminium.	
0	0	99·93	0·07	0	1·91	98·09	0
1	0·30	99·61	0·09	15·05	1·60	83·35	14·75
2	0·61	99·24	0·15	31·98	1·97	66·05	31·37
3	0·80	99·01	0·19	43·15	2·27	54·58	42·35
4	1·13	98·59	0·28	53·85	2·12	44·03	52·72
5	1·31	98·40	0·29	58·43	1·88	39·69	57·12
6	1·74	98·10	0·16	66·40	2·10	31·50	64·66
7	2·32	97·47	0·21	71·07	2·83	26·10	68·75
8	3·16	96·53	0·31	75·33	2·63	22·04	72·17
9	5·19	94·61	0·20	80·37	2·85	16·78	75·18
10	6·69	93·18	0·13	84·34	3·28	12·38	77·65
11	7·87	91·99	0·14	84·63	3·83	11·54	76·76
12	11·75	88·14	0·11	84·91	5·40	9·69	73·16
13	20·99	78·87	0·14	83·10	9·73	7·17	62·11
14	28·15	71·55	0·30	81·17	13·47	5·36	53·02
15	32·82	66·77	0·41	77·64	17·75	4·61	44·82
16	36·40	62·85	0·75	74·65	21·65	3·70	38·25
17	40·73	58·31	0·96	72·88	23·76	3·36	32·15

Fig. 3 represents the critical curve plotted from these values; obviously no marked irregularity of contour is visible, nor any tendency towards convergence of tie-lines to a fixed point, suggesting that aluminium and silver do not form definite atomic compounds analogous to Ag_4Zn_5 and $AgZn_5$ on the one hand, or to $AlSn_4$ on the other. The tie-lines uniformly slope downwards to the left, *i.e.*, the lighter alloy always contains more silver than the heavier one, precisely as in the case of zinc-lead-silver alloys.

The inner dotted curve marked No. 1 represents the curve obtained, as above described, with tin-aluminium-lead alloys, obviously underlying that obtained with silver-aluminium-lead alloys, notwithstanding that the temperature in the latter case was somewhat higher, about 870° as compared with 800°.

The inner dotted curve, marked No. 2, similarly represents the curve obtained with zinc-silver-lead alloys (Part V), omitting the



bulges observed due to the formation of the definite compounds $AgZn_5$ and Ag_4Zn_5; a comparison of this with the aluminium-silver-lead curve shows that the substitution of aluminium for zinc greatly raises the curve, as previously found with aluminium-tin-lead and aluminium-tin-bismuth, as compared with zinc-tin-lead and zinc-tin-bismuth alloys respectively.

On applying Stokes' second method to the above figures, a large-scale plotting leads to the values $A+A' = 80$, $B+B' = 3·5$ at the limiting point; whence the composition for this point—

Lead	$\frac{80}{2}$	=	40·0
Aluminium........	$\frac{3·5}{2}$	=	1·75
Silver............	—	=	58·25
			100·00

representing a ratio between aluminium and lead close to that indicated by the formula Pb_3Al. The corresponding ratio found with aluminium-lead-tin alloys, as above described, was near that indicated

by $PbAl_7$, whilst zinc-lead-silver alloys gave a ratio near that indicated by the formula Pb_2Zn (Part V).

	Calculated.	Found.		
Pb............	95·8	40·0	=	95·8
Al............	4·2	1·75	=	4·2
	100·0	41·75		100·0

Mixtures of Aluminium, Bismuth, and Silver.

A series of mixtures of aluminium, bismuth, and silver was prepared in precisely the same way, the temperature during the period of tranquil fusion being the same, viz., between 830° and upwards of 900°, averaging about 870°, or a little higher; for the lower ties, as before, bismuth and aluminium were employed in about the relative proportion 2 to 1, increasing quantities of bismuth being used for the upper ones, until the proportion reached about 10 to 1. The alloys were analysed by dissolving in nitric acid, and precipitating the silver as chloride, and diluting the filtrate and precipitating with sulphuretted hydrogen; the sulphide of bismuth was ultimately weighed as oxide, after dissolving in nitric acid and precipitating with ammonium carbonate; whilst the aluminium was precipitated from the sulphuretted hydrogen filtrate by ammonia, and the weighed alumina corrected for silica and ferric oxide, as before; the results being in all cases calculated upon the sum of silver, bismuth, and aluminium found as 100.

The following table represents the results obtained by the examination of 20 compound ingots, representing 40 alloys :—

No. of tie-line.	Heavier alloy.			Lighter alloy.			Excess of silver percentage in lighter alloy over that in heavier.
	Silver.	Bismuth.	Aluminium.	Silver.	Bismuth.	Aluminium.	
0	0	99·72	0·28	0	2·02	97·98	0
1	0·74	98·39	0·87	12·30	2·66	85·04	11·56
2	1·59	97·43	0·98	24·20	3·49	72·31	22·61
3	1·95	97·31	0·74	33·95	3·50	62·55	32·00
4	2·01	97·49	0·50	51·80	3·39	44·81	49·79
5	4·81	94·72	0·47	68·63	3·94	27·43	63·82
6	11·51	88·09	0·40	81·29	4·71	14·00	69·78
7	21·56	77·72	0·72	83·75	6·45	9·80	62·19
8	25·41	74·07	0·52	83·88	7·98	8·14	58·47
9	31·05	68·28	0·67	82·60	9·96	7·44	51·55
10	40·92	57·97	1·11	75·64	18·70	5·66	34·72
11	48·53	49·62	1·85	68·16	28·50	3·34	19·63

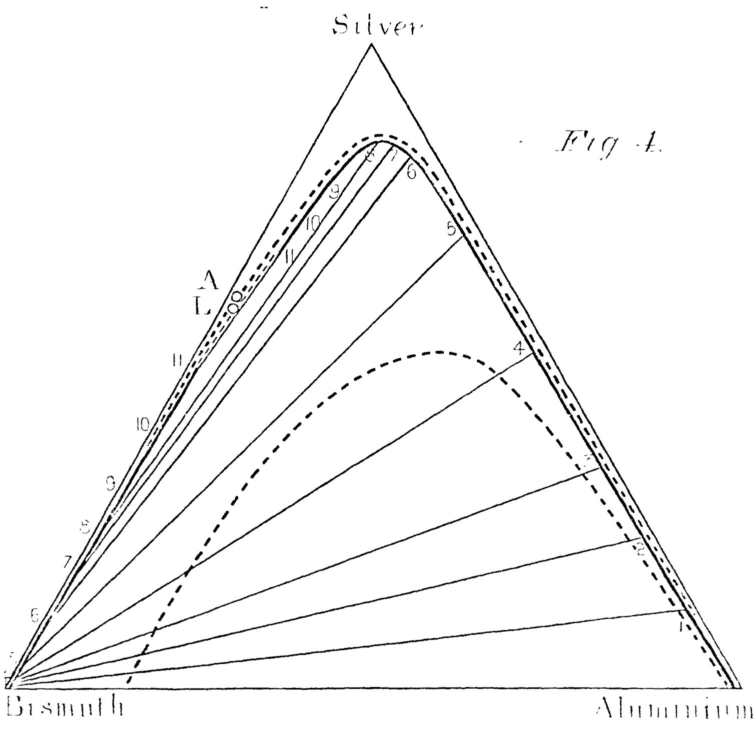


Fig. 4 represents the critical curve deduced from these values; the point marked A represents a mixture of metals that did not separate into two ("real" alloy, containing Ag = 59·9, Bi = 37·3, Al = 2·8).

The outer dotted line is the corresponding curve, obtained as above described, with aluminium-lead-silver alloys, distinctly illustrating the raising of the curve brought about by the substitution of lead for bismuth with aluminium and silver, just as previously found for zinc and tin, zinc and silver, and aluminium and tin.

The inner dotted curve is that obtained with zinc-bismuth-silver alloys (Part V), omitting the bulges, illustrating the effect of substituting aluminium for zinc in raising the curve, just as in the previous cases with lead and tin, bismuth and tin, and lead and silver.

Obviously no marked irregularity of contour is noticeable, nor any change of direction of tie-lines, nor any tendency to converge to a point, apparently indicating the absence of any tendency of aluminium to form definite compounds with silver, like those formed by zinc with silver or tin.

All the tie-lines slope to the *left*, just as in the case of the other

silver-containing ternary alloys examined (silver-lead-zinc, silver-bismuth-zinc, and silver-aluminium-lead), *i.e.*, the lighter alloy always contains the most silver. With tin as solvent metal, on the other hand, the upper tie-lines always slope to the *right*, *i.e.*, the heavier alloy contains the most tin; with bismuth-zinc-tin and bismuth-aluminium-tin alloys this direction of slope is observed with all the tie-lines; but with lead-zinc-tin and lead-aluminium-tin the lower ties slope to the left, apparently because of the formation of definite compounds $SnZn_4$ and $SnAl_4$ on the one hand, and $SnPb_3$ on the other, the influence of the former of which on the bismuth-containing alloys is not perceptible, on account of the lower situation of the curve.

On applying Stokes' second system to the above figures, a large-scale plotting leads to the values at the limiting point $A+A' = 79$, $B+B' = 4{\cdot}5$; whence the composition

Bismuth..........	$\frac{79}{2}$	=	39·50
Aluminium........	$\frac{4{\cdot}5}{2}$	=	2·25
Silver............	—	=	58·25
			100·00

representing a ratio between bismuth and aluminium near to that indicated by the formula Bi_2Al.

	Calculated.	Found.		
Bi..............	93·9	39·50	=	94·6
Al..............	6·1	2·25	=	5·4
	100·0	41·75		100·0

The corresponding ratio found previously in the case of bismuth-zinc-silver alloys was near that indicated by the formula $BiZn_2$ (Part V); whence it would seem that, although the composition of the alloys corresponding with certain points on the analogous curves obtained where aluminium is substituted for zinc exhibit some degree of parallelism (*e.g.*, those corresponding with the ratios $SnZn_4$ and $SnAl_4$), yet this does not apply to the composition represented by the limiting points; and, further, the composition at the limiting point corresponds with a ratio between the two immiscible metals which varies in each case with the nature of the solvent metal. Thus in the eight cases hitherto investigated the ratios approximately found are as follows:—

Immiscible metals.	Solvent metal.	Approximate ratio.
Lead and zinc	Tin	$PbZn_6$
" "	Silver	Pb_2Zn
Lead and aluminium	Tin	Pb_2Al_7
" "	Silver	Pb_3Al
Bismuth and zinc	Tin	$BiZn_{10}$
" "	Silver	$BiZn_2$
Bismuth and aluminium..	Tin	$BiAl_{10}$
" "	Silver	Bi_2Al

The author has much pleasure in acknowledging the assistance of Mr. Sydney Joyce in carrying out a large proportion of the analytical work requisite for the experiments above described.

VI. "The Conditions of the Formation and Decomposition of Nitrous Acid." By V. H. Veley, M.A., University Museum, Oxford. Communicated by Professor Odling, F.R.S. Received April 12, 1892.

Introductory.

Throughout the whole science of chemistry there is possibly no reagent so frequently represented as taking part in various transformations, but of which so little definite is known, as nitrous acid. In many text-books its properties are cursorily discussed in a few lines, while some writers have gone so far as to deny its existence altogether even in the presence of nitric acid. Among the commoner examples of reactions considered to be effected by nitrous acid, it is necessary only to mention the conversion of the primary paraffinoid amines into the corresponding alcohols, the formation of nitroso- and diazo-derivatives, and the preparation of the fulminates. In a previous paper* it was my endeavour to prove that the solution of certain metals in nitric acid was conditioned by the presence of nitrous acid, and the cause of the chemical change explained on the supposition that the acid is alternately formed and decomposed.

As a fitting corollary to these investigations, it seemed worthy of interest to examine to some extent the validity of this hypothesis by endeavouring to imitate the reactions supposed to take place on solution of the metal, either when no metal is present, or when the metallic salt is either present or absent.

The stability of nitrous acid in presence of nitric acid forms also a part of this research, and, conversely, the stability of nitric acid in absence of nitrous acid is discussed.

In the present, as in my former, investigations, it will be under-

* 'Phil. Trans.,' 1891, A, pp. 312—313.

stood that the term nitrous acid is applied to that kind of matter which decolorises potassium permanganate, liberates iodine from potassium iodide, and gives various colour reactions with certain organic bases.

The Formation of Nitrous Acid in Nitric Acid Solution.

Nitrous acid is generally produced directly by the decomposition or reduction of nitric acid effected (i) by heating the acid, (ii) by passing nitric oxide or nitrous fumes into it, (iii) by electrolysis, or more indirectly (iv) by addition of nitrogen peroxide to water, and (v) by decomposition of metallic nitrites with acids. The nature of these several changes, their conditions, and the composition of the solutions obtained form the main subjects of this investigation.

The Methods of Analysis.

The following process was adopted for estimating the respective amounts of nitrous and nitric acids in presence of one another; the total acidity was determined in the usual manner by means of standard alkali, the nitrous acid by means of potassium permanganate; from the factors thus obtained the amount of acidity due to the nitric acid by itself could be readily calculated. It was found by experience not only in the present but also in previous investigations that nitrous acid could not conveniently be estimated by the addition of the permanganate solution until it is no longer decolorised, for the oxidation of the nitrous acid at the end of the operation is gradual and not instantaneous. This point does not seem to have been noticed by the writers on quantitative analysis. Accordingly the method was modified as follows:—The solution of the nitrous acid was added to such a quantity of the standard permanganate, acidified with sulphuric acid, which was judged to be in slight excess over that required to complete the oxidation, and the whole mixture allowed to stand in stoppered bottles for half an hour. The pink liquid was then poured into potassium iodide solution, and the amount of iodine liberated by the excess of the permanganate determined, as customary, by standard sodium thiosulphate solution. There was apparently no risk of any of the nitrous acid escaping oxidation, and thus liberating iodine from the potassium iodide, if the method was carried out as described.

In order to test the accuracy of the process, some purified silver nitrite was recrystallised several times from water, a known quantity of it was weighed out, suspended in water, and decomposed by a slight excess of purified sodium chloride. The amount of nitrous acid calculated as (NO_2) was determined in the solution, and for a test analysis the following may be cited :—

Amount of (NO_2) in 1 c.c. calculated from the weighed quantity of silver nitrite taken..............................	0·005963 gram.
Amount of (NO_2) in 1 c.c. found	0·005974 „

The process is, therefore, accurate in this particular case to within 1 part in 600. When smaller quantities of nitrous acid were to be estimated, the metaphenylenediamine method was adopted, the coloration produced being compared with a solution containing a known quantity of nitrous acid by means of the form of tintometer described in my former paper (*vide supra*).

The Stability of Nitric Acid.

It seemed worthy of investigation to determine the actual temperature at which traces of nitrogen peroxide begin to be formed in purified nitric acid of different degrees of concentration; hitherto only general statements are given in the text-books.* Samples of acid of sp. gr. 1·5 and 1·4 respectively were purified by the method of blowing a current of air at a temperature of 35°, and thus obtained quite colourless; the latter was diluted (i) with its own volume, and (ii) with three times its volume, of water. An acid of sp. gr. 1·53 was prepared by distillation of recrystallised nitre with sulphuric acid, and the reddish-yellow acid thus obtained was redistilled with an equal bulk of sulphuric acid. A number of attempts were made to render this acid colourless by a current of air or carbonic acid, but, notwithstanding several modifications, these proved unsuccessful. Recourse was, therefore, had to distillation *in vacuo* in an apparatus specially constructed of glass.

The method of operation was as follows:—The slightly yellow acid was placed in the distillation flask, and the receiver kept cooled by a freezing mixture; at a temperature of 45°, and under a pressure of 15 mm., the acid passed over without any visible ebullition. After one-third of the liquid had been condensed, the receiver was changed, and the remainder distilled over; the first portions contained a greater part of the nitrogen peroxide. The second distillate was then redistilled in the same manner, and thus a perfectly colourless liquid was obtained (sp. gr. 0/0 = 1·541)† containing only 0·011 milli-

* Carius ('Ber. Deut. Chem. Ges.,' 1870, p. 690) states that when pure nitric acid of sp. gr. 1·51 is heated in sealed tubes to a temperature of 150° and upwards, a reddish-yellow liquid is produced, with evolution of oxygen and "nitrous acid gas;" above 250° the liquid contains such an abundance of nitrous acid that it gives a blue colour on addition of a small quantity of water, and on further dilution nitric oxide is evolved. Accordingly, the decomposition of the acid under these conditions is expressible by the equation $2HNO_3 = N_2O_3 + O_2 + H_2O$.

† I am indebted to my colleague Dr. J. Watts for loan of apparatus and assist-

gram of nitrogen peroxide. Attempts to prepare an acid of greater concentration and more free from the peroxide proved unsuccessful.

In the following table are given the amount of nitric acid in 1 c.c. of each of the several samples.

Sample.	Amount of nitric acid in 1 c.c.	Specific gravity 10/10.
I	1·350 grams	1·541
II	1·1890 „	1·512
III	1·0763 „	1·420
IV	0·5183 „	not determined
V	0·2563 „	„

For each experiment 10 c.c. of the acid were placed in a small piece of combustion tubing, which had previously been cleansed by being filled with concentrated nitric acid and allowed to stand for several hours, generally over night. The tubes were then rinsed several times with water, and finally with water redistilled from potassium permanganate. It was hoped that by this method all reducing substances might be completely destroyed. After introduction of the acid the tubes were quickly sealed up and then heated to various temperatures in water or paraffin baths. The experiments were conducted in dull and generally foggy weather, advantageous at least for them, as concentrated nitric acid is decomposed by direct sunlight. At the end of each experiment the 10 c.c. of acid were poured into 100 c.c. of water, and the amount of nitrous acid determined by the metaphenylenediamine method, as explained above; this is then reckoned in terms of nitrogen peroxide, the substance which imparts the yellow tint to the impure acid.

Sample of acid.	Temperature.	Time.	Nitrogen peroxide produced expressed as 0·001 milligram in 1 c.c.
I	30°	90′	0·25
III	„	„	2·9
IV	„	„	0·51
V	„	„	nil

ance in this method of procedure, which both in his and my hands has proved more successful than the air (or carbonic acid) current method, usually described in the text books, when acids of specific gravity greater than 1·5 are required perfectly colourless.

The distillation method, however, presents one of two alternate difficulties: on the one hand, in each repetition of the distillation the proportion of nitrogen peroxide is decreased, on the other, that of the water is increased.

These results show that whereas the amount of nitrogen peroxide present in the most concentrated acid is doubled by heating under the conditions described above, yet the amounts of this same impurity produced in the less concentrated acids are quite inappreciable. A series of other determinations were made at high temperatures, the results of which are given below.

Sample of acid.	Temperature.	Time.	Nitrogen peroxide present. 0·001 milligram in 1 c.c.
I	58°	90′	0·27
III	”	”	2·7
IV	”	”	2·9
V	”	”	not measurable

Sample of acid.	Temperature.	Time.	Nitrogen peroxide present.
I	100°	90′	0·431
III	”	”	0·05
IV	”	”	nil
V	”	”	nil

Sample of acid.	Temperature.	Time.	Nitrogen peroxide present.
II	120°	90′	0·96
III	”	”	0·2
IV	”	”	0·2
V	”	”	nil

Sample of acid.	Temperature.	Time.	Nitrogen peroxide present.
III	155°	90′	0·70
IV	”	”	0·06
V	”	”	0·06

Sample of acid.	Temperature.	Time.	Nitrogen peroxide present.
III	195°	5′	219
IV	”	”	80

It is manifest from the figures given in the tables above that sample No. I was decomposed to some extent at 58°, and to a greater degree at 100°, sample No. II at 120°, sample No. III at 155°, and very rapidly at 195°, sample No. IV at 195°, while the most dilute acid remained practically unaltered throughout. It would also appear that, provided every care be taken to eliminate reducing substances, especially of an organic character, and provided also that the acids are not unduly exposed to sunlight, nitric acid is more stable than former experience indicated. It would be an interesting subject of enquiry to ascertain as to whether nitric acid is affected by shocks, as I have found that samples of acid purified so as to be practically free from nitrous acid were contaminated with that impurity after a railway journey.

Formation of Nitrous Acid from Nitric Oxide and Nitric Acid.

It is a common matter of observation that when nitric oxide gas is passed into nitric acid a greenish-blue solution is produced, which shows all the reactions of nitrous acid; some writers, however, consider that the nitric oxide gas is only dissolved as such in the nitric acid in that, when the liquid is warmed, the gas is again evolved. It is more probable that nitrous acid is formed and subsequently decomposed, changes represented by the following equations :—

$$\text{(I.)}\ 2NO+HNO_3+H_2O = 3HNO_2.$$
$$\text{(II.)}\ 3HNO_2 = 2NO+HNO_3+H_2O.$$

Clemente Montemartini* has proved that of these changes the latter proceed quantitatively, at least in dilute solution, in accordance with the equation given.

If these changes are strictly reversible, it would follow as a necessary consequence that there would be a limit to the amount of the nitrous acid produced, and this limit would be dependent upon conditions such as concentration of acid and temperature. The equation for equilibrium will be

$$p/q = \text{constant},$$

wherein p is the mass of nitric acid, and q is the mass of nitrous acid, for when the limit is reached the amounts of nitric oxide gas passing in and out would be identical, while the alteration of the mass of water would be immaterial.

On the other hand, if the phenomenon is merely that of solution, the amount of nitric oxide gas dissolved should, other conditions remaining the same, diminish under all circumstances as the temperature increases.

* 'Rome, Accad. Lincei Rendiconti,' 1890, p. 264.

To decide between these views, and to determine the limit, whether of nitrous acid formed or of nitric oxide dissolved, as the case might be, a series of experiments were conducted in the following manner.

Nitric acid was passed through an apparatus consisting of (1) a wash-bottle containing soda; (2) a set of Liebig's bulbs; and (3) a set of Geissler's bulbs both filled with nitric acid of the same concentration, the latter of which was used for the analytical determinations; and (4) a wash-bottle containing soda to prevent any nitrous fumes, produced by the passage of the unaltered nitric oxide into the air, from accidentally coming in contact with the acid experimented upon. In these and succeeding experiments the apparatus consisted wholly of glass, except for small pieces of rubber tubing which served for connections; these were frequently renewed. Before the nitric oxide was passed, the whole apparatus was filled with hydrogen, my previous experiments having shown that this gas, when ready-made, does not reduce nitric acid, but even mechanically blows off any slight impurity of nitrous acid. At the end of the operation the Geissler's bulbs were quickly detached, and the acid blown out by a rapid stream of carbonic acid. While the experiment was proceeding the bulbs were kept in a water-bath, the temperature of which was carefully regulated; the passage of the nitric oxide was kept as uniform as possible.

The following table contains the results of the first set of experiments; not only of the actual amounts of nitric acid, and of nitrous acid, *i.e.*, the reducing power, are given, but also of the percentage amounts of these acids, taking the total acidity as 100:—

Series I.

Temperature, 22·5°.

	Time.	Nitric acid in 1 c.c.	Nitrous acid in 1 c.c.	Percentage rates.
	hours.	gram.	gram.	
—	—	0·2549	nil	100 : 0
After passage of nitric oxide	4	0·2404	0·0168	93·47 : 6·53
" " "	8	0·2409	0·0164	93·62 : 6·38

These results show that the phenomenon, whether of reaction or of solution, was complete at the end of the first interval of time; but, taken by themselves, they would not suffice to distinguish under which category the change is to be classified. Accordingly a series of experiments were conducted in which the only variable condition was that of temperature, for, as pointed out above, if the nitric oxide

is merely dissolved, then the reducing power would diminish with increase of temperature, but if a chemical change takes place, then its amount would depend upon the relative intensities of the chemical change represented by the equations given.

Series II.

Temperature, 9°.

	Time.	Nitrous acid in 1 c.c.	Nitrous acid in 1 c.c.	Percentage rates.
	hours.	gram.	gram.	
—	—	0·2549	nil	100 : 0
After passage of nitric oxide	3	0·2496	0·0169	93·66 : 6·34

Series III.

Temperature, 27·5°.

	hours.	gram.	gram.	
—	—	0·2549	nil	100 : 0
After passage of nitric oxide	4	0·2336	0·0171	93·18 : 6·82
" " "	8	0·2362	0·0169	93·69 : 6·31

Series IV.

Temperature, 32°.

	hours.	gram.	gram.	
—	—	0·2549	nil	100 : 0
After passage of nitric oxide	3	0·2318	0·0223	91·22 : 8·76

Series V.

Temperature, 42°.

	hours.	gram.	gram.	
—	—	0·2549	nil	
After passage of nitric oxide	3	0·2372	0·016	93·68 : 6·32

Series VI.

Temperature, 52°.

	hours.	gram.	gram.	
—	—	0·2549	nil	
After passage of nitric oxide	3	0·2366	0·0136	94·56 : 5·44

It will be evident from the above experiments that the ratio of nitrous to nitric acid increases slightly with rise of temperature up to 32°, but from this point it decreases so that the values at 9° and 42° are nearly identical.

These results, in the case of acid of the concentration used, would seem to indicate that the phenomenon is not entirely one of solution, but partly also of reversible chemical changes, the difference in their relative intensities being greatest at or about 32°, and least at 52°, a temperature at which nitrous acid by itself would be very rapidly decomposed. These results were also confirmed by experiments conducted with a sample of acid of half the concentration of that used above.

Concentration of nitric acid = 0·1279 gram.

Series VII.

Temperature, 22°·5.

Time.	Nitric acid in 1 c.c.	Nitrous acid in 1 c.c.	Percentage ratio.
hours.	gram.	gram.	
3	0·1257	0·0124	89·79 : 10·21
4	0·1276	0·0136	90·37 : 9·63

Series VIII.

Temperature, 32°.

hours.	gram.	gram.	
3¼	0·1203	0·0137	89 78 : 10·22

Series IX.

Temperature, 42°.

hours.	gram.	gram.	
3	0·1185	0·0114	91·22 : 8·78

Series X.

Temperature, 52°.

hours.	gram.	gram.	
3	0·1274	0·0088	93·54 : 6·46

In these sets of experiments, also, the amount of nitrous acid formed increases up to a temperature of 32°, and from this point again decreases. Further from comparison of results obtained with the two samples of acid at the same temperature it is evident the percentage ratio of nitrous acid is *increased* with *decrease of concentration.* To confirm this a further series of experiments were conducted with an acid of one-fourth of the concentration of that used in the first set.

Concentration of acid = 0·0655 gram in 1 c.c.

Series XI.

Temperature, 22°·5.

Time.	Nitric acid in 1 c.c.	Nitrous acid in 1 c.c.	Percentage ratio.
hours.	gram.	gram.	
4½	0·0662	0·0097	87·22 : 12·78
2	0·0661	0·0088	88·25 : 11·75

Series XII.

Temperature, 32°.

hours.	gram.	gram.	
3	0·0624	0·0097	86·55 : 13·45
2	0·0634	0·0105	85·79 : 14·21

Series XIII.

Temperature, 42°.

hours.	gram.	gram.	
3	0·0630	0·0081	88·61 : 11·39

Series XIV.

Temperature, 52°.

hours.	gram.	gram.	
3	0·0652	0·0062	91·31 : 8·69

The results given in the four tables above are perfectly in accordance with those of the preceding series, the maximum value for the

amount of nitrous acid being as before at a temperature of 32°; they also show that with decrease of concentration the percentage ratio of nitrous acid is increased, though its total amount is decreased. Experiments were then conducted with acids of greater concentration, the results of which are given below.

Concentration of acid = 0·3457 gram in 1 c.c.

Series XVI.

Temperature, 22°.

Time.	Nitric acid in 1 c.c.	Nitrous acid in 1 c.c.	Percentage ratio.
hours. 3	gram. 0·3254	gram. 0·0268	92·39 : 7·61

Series XVII.

Temperature, 32°.

hours. 3	gram. 0·3253	gram. 0·0212	93·88 : 6·12

Series XVIII.

Temperature, 42°.

hours. 3	gram. 0·3080	gram. 0·0173	94·68 : 5·32

Concentration of acid = 0·6338 gram in 1 c.c.

Series XIX.

Temperature, 22°·5.

Time.	Nitric acid in 1 c.c.	Nitrous acid in 1 c.c.	Percentage ratio.
hours. 3	gram. 0·5940	gram. 0·0397	93·74 : 6·26
3 (expt. repeated).	0·5791	0·0408	93·42 : 6·58

Series XX.

Temperature, 32°.

hours. $3\frac{1}{2}$	gram. 0·5306	gram. 0·0295	94·74 : 5·26

The liquids obtained in the experiments detailed above were of a *blue* tint, but it was also observed that in the case of the more concentrated acids red fumes were evolved to a slight extent, thus showing under these conditions a more complex reaction than the simple reversible change given above. It is also to be noticed that the sum of the quantities of nitrous and nitric acids found are less than the quantity of nitric acid originally taken, thus showing that some unaccounted-for destruction of the acid had taken place. In a set of experiments with more concentrated acids the evolution of fumes and the destruction of the acid were further noticeable.

Concentration of acid = 1·089 gram in 1 c.c.

Series XXI.

Temperature, 22°·3.

Time.	Nitric acid in 1 c.c.	Nitrous acid in 1 c.c.	Percentage ratio.
hours.	gram.	gram.	
$1\frac{1}{2}$	0·8704	0·0632	93·25 : 6·75
2 (expt. repeated)	0·8700	0·0664	92·91 : 7·09

Series XXII.

Temperature, 27°.

hours. $1\frac{1}{2}$	gram. 0·7009	gram. 0·0391	94·72 : 5·29

In the above series of experiments, when the nitric oxide gas was first passed in a yellow colour was produced, but this speedily changed into a deep *green* tint. It thus appeared that nitrogen peroxide was at first produced and this was converted, possibly by the nitric oxide and possibly, also, by the water present, into the green acid which certain writers have considered to have the composition $N_2O_3.N_2O_3.H_2O$;

such an acid, however, would contain nearly 50 per cent. of nitrous acid.

After most of the experimental work above detailed had been performed, a paper appeared by F. Marchlewski,* entitled "Zur Kenntniss der verschiedenen Färbungen der Salpetersäure," in which the author describes various experiments upon the reaction between nitric oxide and nitric acid. The main object of the paper was to determine the composition of the various coloured liquids produced when nitric oxide, nitrous fumes, and nitrogen peroxide are passed into nitric acid of different concentration. The method employed consisted in preparing such liquids and then decolorising them by a current of carbonic anhydride; the gases evolved were passed into sulphuric acid and the solution subjected to analysis. The process is based upon the observations of Lunge that nitrous fumes give under these conditions nitrosyl sulphuric acid, while nitrogen peroxide gives a mixture of nitrosyl sulphuric and nitric acids, thus:—

$$\text{(I.)}\quad 2H_2SO_4+N_2O_3 = 2SO_2.NO_2.OH+H_2O.$$
$$\text{(II.)}\quad H_2SO_4+N_2O_4 = SO_2.NO_2.OH+HNO_3.$$

It might appear open to question whether the composition of these liquids can be ascertained by this indirect method, and whether a liquid apparently decolorised contains nothing but the residual nitric acid (more or less diluted) of the operation. My own experience has shown that it is extremely difficult to remove the last traces of the yellow colour from the more concentrated acids; though, on the other hand, it must be allowed that the method of analysis adopted in the text would not distinguish between nitrous acid on the one hand and nitrogen peroxide on the other, the latter of which would appear in the course of analysis as an equimolecular mixture of nitrous and nitric acids. These remarks would, however, apply probably only to the two last series of experiments. Marchlewski gives no observations of time, temperature, or degree of humidity of gases, whether oxides of nitrogen or carbonic anhydride, nor a single control analysis of the residual nitric acid. Some proof is given that the difference between the *green* and *blue* acids is dependent upon not only the water present but also upon the dissolved nitric oxide. Marchlewski seems to be quite unaware of the observations of Péligot,† made nearly forty years ago, upon the same subject.

* 'Ber. Deut. Chem. Ges.,' vol. 24, p. 3271.

† 'Ann. Chim. Phys.' [III], vol. 2, p. 58. The remarks of Péligot seem worth transcribing, as having possibly fallen into undeserved oblivion: "Comme l'acide azoteux pur paraît être bleu tandis que l'acide azotique et l'acide hypoazotique mélangés sont jaunes, on obtint par cette action de l'eau" (viz., upon nitrogen peroxides) "selon les proportions employées les différents nuances de vert et de bleu

Certain experiments were made upon the action of dry nitric oxide upon nearly anhydrous nitric acid (sp. gr. 7/7° = 1·5326), the preparation of which has been described in a previous section. The air was driven out of the apparatus by a current of carbonic acid dried by passage through a wash-bottle containing concentrated sulphuric acid, and then through three drying tubes filled with pumice and phosphorus pentoxide. The nitric acid was contained in the Geissler's bulbs as before (kept at a temperature of 11°), and subsequent to them was another wash-bottle of sulphuric acid to prevent the access of moisture by backward diffusion.

On passage of the nitric oxide gas, the portion of acid in the first bulb was turned a yellow tint, which gradually deepened to an orange-red; when about a litre of the gas had been driven through a red oil appeared floating upon the surface of the acid, and a few minutes after the red oil turned a green tint, and for some time there were two distinct liquids of different densities in the bulb, but afterwards a green uniform liquid was obtained. Meanwhile the portion of acid in the second bulb had deepened in tint, then the red oil was formed, and the other phenomena in due succession. (The weight of the acid at this point had increased.) When about 4 litres of nitric oxide had passed in the green liquid in the first bulb gradually turned to a blue, which slowly evaporated into the second bulb, and eventually only a few drops of an almost colourless liquid were left. Simultaneously the portion of acid in the third bulb showed the transition tints of yellow to orange with ultimate formation of the red oil, and also dense red fumes passed out of the acid. At this point the experiment was stopped, and the weight of the nitric acid was found to have considerably decreased.

The explanation of these changes, which were observed in part by Priestley* at the end of the last century, seems to be as follows:—At first nitrogen peroxide is formed by the mutual oxidation of the nitric oxide and reduction of the nitric acid, and this is retained in solution by the excess of the acid

$$2HNO_3+2NO = 3N_2O_4+2H_2O.$$

A point is reached at which the acid becomes supersaturated, and the

. . . qui se développent également par l'action du bioxyde d'azote sur l'acide azotique à différents degrés de densité.

"Le produit vert . . se forme aussi par le contact du bioxyde d'azote avec l'acide hypoazotique, et sa production pouvait être tout à fait indépendante de l'eau" (as shown by experiment).

* "Experiments and Observations on different kinds of Air," vol. 1, p. 383. I may be allowed to allude to the section of this work relating to nitrous acid, which contains a number of observations upon its properties; several of them appear to have been published as novelties many years afterwards.

excess of the peroxide separates out (the red oil). According to the experiments of Péligot, nitric oxide passed into this gives a green liquid. The absorption and retention of the nitrogen peroxide accounts for the gain in weight (about one-sixth) of the acid. The reduction then proceeds further, and nitrous acid (the blue liquid) is produced, thus:—

$$N_2O_4+2NO+2H_2O = 4HNO_2,$$

and this is evaporated by the continued current of the nitric oxide, leaving finally only the water originally present, both free and combined, in the nitric acid. This destruction of the nitric acid accounts for the loss in weight. As Priestley wrote: "Towards the end of the process (of absorption of nitrous air by pale-yellow spirit of nitre), the evaporation of the acid was perceived to be very great, and when I took it out the quantity was found to be diminished by one-half. Also, it had become, by means of the process and evaporation together, exceedingly weak, and was rather blue than green."

Reaction between Nitric Oxide and Mercuric Nitrate in Nitric Acid Solution.

In my former paper* it was shown that when a current of hydrogen was passed through a solution of mercuric nitrate in nitric acid solution, the mercuric was reduced to a mercurous salt without any considerable formation of nitrous acid, which was, however, produced in abundance when cupric nitrate was subjected to the same action. As it seemed probable that the nitrous acid which might have been produced in the former operation was in reality used up in the reduction of the mercuric salt, the experiment was repeated in precisely similar manner, nitric oxide being substituted for hydrogen. It was found, as before, that the mercuric salt was reduced; from 0·5 gram mercuric oxide dissolved in nitric acid (1 c.c. containing 0·2549 gram acid) after passage of nitric oxide for $3\frac{1}{2}$ hours at a temperature of 35°, 0·019 gram of mercurous chloride was obtained, while from a similar portion, through which the nitric oxide had not been passed, 0·0018 gram was precipitated. It is probable, then, that the hydrogen in the former experiments reduced at first the free nitric into nitrous acid, which in its turn reduced the mercuric nitrate, thus:—

$$2Hg(NO_3)_2+2HNO_2 = Hg_2(NO_3)_2+2HNO_3.$$

No free nitrous acid would therefore appear in the course of the operation.

* 'Phil. Trans.,' 1891, A, p. 315.

Summary of Results.

I. The reaction between nitric oxide and nitric acid varies with the concentration of the acid and the temperature; with more concentrated acids nitrogen peroxide is at first formed and then nitrous acid; with less concentrated acids the latter is produced at once.

II. Only with quite dilute acids (of 30 per cent. concentration and below) does the reaction between the nitric oxide and nitric acid appear to be reversible; the average value for $p/q = 9$ (p = quantity of nitric, q of nitrous, acid), though the actual value varies from 3 to 4 per cent. on either side, according to the conditions of the experiment.

III. With more dilute acids the amount of nitrous acid formed at first increases slightly and then decreases with the temperature; but with more concentrated acids the amount uniformly decreases.

IV. The proportional quantity of nitrous acid formed increases with decrease of concentration, but the actual quantity is of course less.

It would appear that the simple reversible reaction between nitric oxide and nitric acid becomes modified with acids above a concentration of about 30 per cent. and a temperature of 32°. These were also approximately the limits of concentration and temperature, above which the reactions between metals and nitric acid could not be prevented by substances such as urea, potassium chlorate, and the like. I will, however, merely allude to the point without wishing to lay an undue stress upon possibly nothing but chance coincidences.

The Rate of Decomposition of Nitrous Acid.

The velocity of the decomposition of nitrous acid, presumably in nitric acid solution, has been made the subject of a previous investigation by Clemente Montemartini (*vide supra*). As a result of several series of experiments this writer considers that the rate of this decomposition may be expressed by the differential equation

$$\text{(I.)}\ \frac{dC}{dT} = kC \quad \text{or} \quad \text{(II.)}\ k = \frac{1}{T}\log\frac{C_0}{C_1},$$

in which C is the concentration, T the time, and k is a constant.

This equation will be discussed in the sequel. My experiments were commenced primarily with a view of ascertaining if nitrous acid is the more stable the greater the proportion of nitric acid present, a point which was raised in the course of my investigations on the reactions of nitric acid and metals. Further, it seemed probable that the presence of metallic salts might also affect the stability of nitrous acid, as Armstrong and Acworth* allude to the persistence with

* 'Chem. Soc. Journ.,' 1877 (II), p. 54.

which the nitrous acid is retained by solutions containing the products of the reaction between nitric acid and metals. It also seemed possible that such a line of inquiry might throw some light upon the nature of an acid intermediate between nitrous and nitric acids, in whose existence certain writers believe, though the facts adduced at present are perhaps hardly sufficient to warrant such a belief.

The apparatus and method of experiment finally adopted were as follows:—The nitrous acid solution was placed in a cylindrical tube containing 100 c.c. up to a certain mark; at its upper end were two apertures, into one of which was sealed a glass delivery tube, while the other could be wholly or partially closed by a small rubber plug through which passed a pipette of exactly 2 c.c. capacity. The former, which served for the passage of a stream of carbonic acid, was sealed off at its lower end, the gas passing through a small pin-hole at the side, and thus not directly impinging upon the liquid; the space between the loosely-fitting plug and the glass wall serving for its exit. At the end of certain intervals of time the plug was rammed in, whereby the pressure of the gas forced the liquid up the pipette; when this was filled up to the graduation mark the sample was removed for analysis, while the aperture of the cylindrical tube was partly closed meanwhile by a duplicate plug. The cylinder was immersed in a water-bath, the temperature of which was kept constant to within one-tenth of a degree on either side of that required. Before any observations were made the ratio of nitrous to nitric acid present was determined.

Nitrous Acid from Silver Nitrite and Hydrochloric Acid.

The solution of nitrous acid was prepared in the usual manner by the decomposition of recrystallised silver nitrite with a slight deficiency of hydrochloric acid, and the liquid filtered from the precipitated silver chloride into the decomposition flask. The amount of nitrous acid was determined at the end of fixed intervals of time, generally half an hour, and the results calculated on the hypothesis that the volume of the liquid was maintained constant throughout the course of the experiment.

In the following table the results obtained are set forth, in which T is the time from start, and C the concentration of the solution.

Series XXIII.

Volume of solution = 60 c.c. Temperature, 24° 9±0·1.

Ratio of nitrous to nitric acid = 1 : 3·86.

T.	C.	T.	C.
′	gram.	′	gram.
0	0·3063	120	0·2306
30	0·2793	150	0·2130
60	0·2559	180	0·2017
90	0·2413	210	0·1890

These results are expressible by the formula

$$\log(\mathrm{T}+t)+\log\mathrm{C} = \log k,$$

in which T is the time from start, and t an interval of time which would have elapsed from the moment at which, conditions remaining otherwise the same, the amount of nitrous acid was infinitely great, namely, $\mathrm{C} = \infty$, and that moment at which the experiment was actually commenced, C is the concentration, and k is a constant.

In the above series the value for t is taken as 320′, and in the table below are given the values for log $(\mathrm{T}+t)$, log c, and log k.

log (T + t).	log C.	log k.
2·5051	$\bar{1}$·4861	1·9912
2·5441	$\bar{1}$·4461	1·9902
2·5798	$\bar{1}$·4080	1·9878
2·6128	$\bar{1}$·3825	1·9953
2·6335	$\bar{1}$·3629	1·9964
2·6627	$\bar{1}$·3283	1·9910
2·6901	$\bar{1}$·3046	1·9947
2·7160	$\bar{1}$·2764	1·9924

The values in the third column show great concordance; if the mean 1·992 be taken and the results calculated therefrom, the observed and calculated values for the concentration of the nitrous acid will compare as follows:—

C (observed).	C (calculated).	C (observed).	C (calculated).
gram.	gram.	gram.	gram.
0·3063	0·3071	0·2306	0·2285
0·2793	0·2808	0·2130	0·2138
0·2539	0·2586	0·2017	0·2006
0·2413	0·2397	0·1890	0·1890

The greatest difference occurs in the third value, which is rather less than 2 per cent., which can readily be accounted for by errors of experiment in dealing with a substance so susceptible of decomposition as nitrous acid. The curve which represents the concentration of the acid in terms of time is a portion of a hyperbola, the differential equation of which is

$$\frac{dC}{dT} = -\frac{C^2}{M},$$

which expresses the rate at which equivalent masses react, whether it be of the nitric with the nitrous acid, or the nitrous acid with itself; in each experiment $1/M$ is the amount of each unit mass which reacts with the other per unit time when an unit mass of each substance is present.

If Clemente Montemartini's equation $dc/dT = kC$ is correct, there should be two reacting substances, one of constant concentration throughout the reaction. It is, however, to be observed that the values given for the constant k in his results differ widely among themselves, often as much as 25 per cent. between the maximum and minimum. Further, all the results in each set of experiments are referred to the first, the C_0 of equation (ii), though there is no especial reason that this analytical determination should be more exact than those subsequent to it. With the general purport, however, of Montemartini's paper I cannot but completely agree, namely, that the rate of decomposition of nitrous acid is dependent upon the tension of the superincumbent nitric oxide, without, however, necessarily committing oneself to the view that herein is presented a "true case of dissociation."

It is worthy of remark that the curve representing the decomposition of nitrous acid is exactly identical with that of a previous case of chemical change investigated by myself, namely, the decomposition of formic acid into carbonic oxide and water,* in which the method of observation was precisely the reverse of that adopted in this research. For, herein, the concentration is determined at equal intervals of time, but in the previous case observations were made of the times required for equal diminution of concentration. The results of other series of experiments are given below, the observed and calculated values for the concentration of the nitrous acid being compared in each instance.

* 'Phil. Trans.,' 1888, A, p. 290.

Series XXIV.

Volume of solution = 70 c.c. Temperature, 25°·5.
Ratio of nitrous to nitric acid = 1 : 2·13.

T.	C (observed).	C (calculated). t = 400 log k = 1·250.
′	gram.	gram.
0	0·4382	0·4448
30	0·4009	0·4147
60	0·3915	0·3877
90	0·3673	0·3640
120	0·3460	0·3430
150	0·3231	0·3244
180	0·2979	0·3075
210	0·2966	9·2924
240	0·2836	0·2851
270	0·2714	0·2661

Series XXV.

Volume of solution = 60 c.c. Temperature, 25°·0.

T.	C (observed).	C (calculated). t = 170. log k = 0·856.
′	gram.	gram.
0	0·4157	0·4237
30	0·3646	0·3606
60	0·3190	0·3134
90	0·2881	0·2774
120	0·2484	0·2489
150	0·2381	0·2250
180	0·2102	0·2209
210	0·1894	0·1901
240	0·1647	0·1679

Series XXVI.

Volume of solution = 70 c.c. Temperature, 24°·6 ±0·1.
Ratio of nitrous to nitric acid = 1 : 20·7.

T.	C (observed).	C (calculated). t = 480. log k = 1·452.
′	gram.	gram.
0	0·6707	0·6742
30	0·6262	0·6270
60	0·5789	0·5899
90	0·5537	0·5547
120	0·5255	0·5255
150	0·5065	0·4968
180	0·4775	0·4719

In all the above series of experiments the observed and calculated results are as nearly approximate as can be expected, considering the great instability of the nitrous acid, which is appreciably decomposing even in the brief interval of time required for the transference of the solution from the decomposition flask into the potassium permanganate. In the last series of experiments quoted above, in which a considerable proportion of nitric acid was purposely introduced, it is to be noticed that the rate of decomposition is decreased, even though the mass of nitrous acid originally present was greater, which should of course produce an exactly opposite result. This would, therefore, seem to indicate that nitrous acid is more stable in the presence of excess of nitric acid.

Nitrous Acid from Nitric Oxide and Nitric Acid.

Nitric oxide gas was passed into concentrated nitric acid until a deep green liquid was obtained; 10 c.c. of this were then slowly introduced into about five times its bulk of water, and the whole volume finally made up to 100 c.c.

Series XXVII.

Volume of solution = 100 c.c. Temperature, $27°{\cdot}7 \pm 0{\cdot}1$.

Ratio of nitrous to nitric acid = 1 : 10·1.

T.	C (observed).	C (calculated). t = 360.
′	gram.	gram.
0	0·8812	0·8910
30	0·8372	0·8227
60	0·7749	0·7637
90	0·7021	0·6967
120	0·6791	0·6684
150	0·6443	0·6290
180	0·5874	0·5805
210	0·5603	0·5627
240	0·5242	0·5347
270	0·4965	0·5093

The curve representing the course of decomposition of the nitrous acid obtained by this method is precisely identical with that of the nitrous acid from the silver nitrite; though the rate of decomposition is somewhat diminished. For if the series above be compared with Series XXV, which lasted for the same duration of time, yet though the mass undergoing change was nearly twice as great, and the temperature 2° higher, both of which conditions would increase the rate, yet there is a difference of only 5 per cent. in the amount of nitrous

acid which has disappeared in the two cases. This result confirms that of the immediately preceding series in showing the increase of the stability of nitrous acid in the presence of nitric acid.

Another set of experiments was conducted, the condition of temperature being varied.

Series XXVIII.

Volume of solution = 100 c.c. Temperature, 11°·1 to 11°·3.
Ratio of nitrous to nitric acid = 1 : 10·72.

T.	C (observed).	C (calculated). t = 860. log k = 1·8811.
′	gram.	gram.
0	0·8910	0·8823
30	0·8716	0·8545
60	0·8467	0·8266
90	0·8046	0·8006
120	0·7815	0·7761
150	0·7595	0·7531
180	0·7337	0·7314
210	0·7033	0·7108
240	0·6759	0·6914
270	0·6710	0·6739
300	0·6454	0·6537

A third set of experiments was also conducted at a higher temperature, other conditions remaining the same.

Series XXIX.

Volume of solution = 100 c.c. Temperature, 31°·2.
Ratio of nitrous to nitric acid = 1 : 10·13.

T.	C (observed).	C (calculated). t = 330. log k = 1·3268.
′	gram.	gram.
0	0·6565	0·6432
30	0·6030	0·5893
60	0·5542	0 5443
90	0·4939	0·4712
150	0·4451	0·4422
180	0·4169	0·4162
210	0·3970	0·3930
240	0·3545	0·3720
270	0·3344	0·3538

On a comparison of the results set forth in the three preceding series of experiments, it appears that the velocity of decomposition of nitrous acid is an exponential function of the temperature, the former

increasing in geometrical as the latter increases in arithmetical proportion. This relation may be expressed by the equation

$$v_t = vk^{(t-t_1)},$$

that is to say, the difference of the logarithms of the amounts of nitrous acid decomposed at the end of a given interval of time will be constant for a constant difference of temperature. This is rendered evident by the following comparison:—

Series.	Temp.	Percentage loss after 270°.	Logarithms.
(I)	11·2°	24·00	1·3802
(II)	27·7	43·01	1·6334
(III)	31·2	49·09	1·6909

The logarithmic differences for 1° are therefore:—

From (I) and (III)	0·0153
„ (I) „ (II)	0·0154
„ (II) „ (III)	0·0167
Mean......	0·0158

The rate of chemical decomposition of the nitrous acid is therefore practically doubled for every 20°, which would give a constant logarithmic difference of 0·0151 for every degree. In this respect this change resembles another, otherwise totally different from it, namely, that between marble and hydrochloric acid,* which varies by a like amount for equal differences of temperature. This relation is also exemplified by another pair of experiments in which 20 c.c. of nitric acid through which nitric oxide gas had been passed were made with water up to a constant volume of 100 c.c.

Series XXX.

Volume of solution = 100 c.c. Temperature, 21°·8.
Ratio of nitrous to nitric acid = 1 : 10·2.

T.	C (observed).	C (calculated). $t = 400$. Const. log. = 1·7307.
′	grams.	grams.
0	1·3247	1·3451
30	1·2522	1·2512
60	1·1188	1·1175
90	1·0948	1·0978
150	0·9992	0·9781
180	0·9111	0·9275
210	0·8688	0·8799
240	0·8254	0·8405
270	0·7895	0·8010

* Spring, 'Zeit. Physikal Chem.,' vol. 1, p. 219.

Series XXXI.

Volume of solution = 100 c.c. Temperature, 31°·1.
Ratio of nitrous to nitric acid = 1 : 10·34.

T.	C (observed).	T.	C (observed).
′	grams.	′	gram.
0	1·4432	190	0·8219
30	1·2868	220	0·7441
60	1·2045	250	0·6495
90	1·0720	270	0·6045
120	0·9679	300	0·5509

Percentage loss after 270′ in series .... 58·05 Logarithm .... 1·76385
Do. do. .. 40·40 " .... 1·60655
Logarithmic difference for 1° .... 0·0168

This difference is practically identical with those given above, though the masses of nitric and nitrous acids present were twice as great in the latter as in the former series of experiments.

Nitrous Acid from Nitrogen Peroxide and Water.

About 1—2 c.c. of nitrogen peroxide were slowly added to 50 c.c. of water, the liquid being kept continually stirred; after the mixture had become uniform the volume was made up to 100 c.c. as before. By this means a dilute solution of nitrous and nitric acids was obtained. The method of experiment was conducted as usual.

Series XXXII.

Temperature, 21°·8.

Nitrous acid present after complete admixture = 1·0663 grams.
Nitric acid " " = 2·3544 grams.
Ratio of nitrous to nitric acid = 1 : 2·12.

T.	C (observed).	C (calculated). t = 1030. Const. log = 1·9819.
′	gram.	gram.
0	0·9181	0·9292
30	0·9151	0·9049
60	0·8952	0·8801
90	0·8597	0·8560
120	0·8373	0·8341
150	0·8171	0·8129
180	0·7819	0·7927
210	0·7618	0·7718
240	0·7538	0·7553
270	0·7381	0·7380

The table below contains the results of another set of experiments with a more dilute solution.

Series XXXIII.

Temperature, 21°·8. Nitrous acid = 0·7292 gram. Nitric acid = 1·5457 grams. Ratio = 1 : 2·02.

T.	C (observed).	C (calculated). t = 500. Const. log. = 1·5587.
′	gram.	gram.
0	0·7292	0·7247
30	0·6811	0·6830
60	0·6401	0·6464
90	0·6166	0·6279
120	0·5843	0·5840
150	0·5570	0·5543
210	0·5095	0·5080
240	0·4856	0·4892
270	0·4736	0·4702
300	0·4502	0·4525

Series XXXIV.

Temperature, 31°·8. Nitrous acid = 2·4557. Nitric acid = 2·2571. Ratio of nitrous to nitric acid = 1 : 0·93.

T.	C (observed).	C (calculated). t = 160. Const. log = 1·5692.
′	grams.	grams.
0	2·2321	2·3180
30	1·9764	1·9077
60	1·6981	1·6858
90	1·4778	1·4833
165	1·1924	1·1413
180	1·1081	1·0937
210	0·9986	1·0024
240	0·9010	0·9271

The greatest difference between the observed and calculated results is shown in the first two experiments, but this can in part be accounted for by the difficulty experienced in introducing the nitrous acid solution into the permanganate before that some of the liquid was projected from the measuring vessel by the rapidly evolved nitric oxide gas. The above three series of experiments show that whether the nitrous acid, produced from nitrogen peroxide and water, is of a concentration of 0·45 per cent., or five times that amount, the rate of

decomposition is in accordance with the mathematical formulæ given, since, as it happened by chance, the Series XXXII to XXXIV were continuous as regards the masses of nitrous acid contained therein.

Nitrous Acid from Nitrous Fumes and Water.

When nitrous fumes, from arsenious oxide and nitric acid, are passed into water at ordinary temperatures of 12—15°, their absorption is apparently very incomplete; a liquid is obtained nearly colourless, and containing 1 to 2 per cent. of free nitrous acid. This solution gives off bubbles of gas very freely, especially when poured from one vessel to another, thus calling to mind the behaviour of solutions of hydrogen peroxide. This very rapid evolution of gas introduced, as explained above, unavoidable errors in the method of experiment adopted; there is therefore not so complete an accordance between the observed and calculated values as in the other series of experiments.

Series XXXV.

Temperature, 31°·7. Nitrous acid = 0·7685 gram. Nitric acid = 0·6753 gram. Ratio of nitrous to nitric acid = 1 : 0·88.

T.	C (observed).	C (calculated). t = 160. Const. log = 1·0927.
′	gram.	gram.
0	0·7685	0·7697
60	0·5665	0·5628
90	0·4828	0·4953
170	0·3898	0·3752
200	0·3501	0·3439
230	0·3342	0·3174
260	0·2963	0·2948

The rate of decomposition is nearly identical with that of Series XXXIV, conducted at the same temperature, and in which the ratio of nitrous to nitric acid was also nearly identical.

Series XXXVI.

Temperature, 19°·8. Nitrous acid = 1·1789 grams. Nitric acid = 0·3973 gram. Ratio of nitrous to nitric acid = 1 : 0·34.

T.	C (observed).	C (calculated). t = 160. Const. log = 1·2561.
′	grams.	grams.
0	1·1789	1·200
30	0·9680	1·0008
60	0·8453	0·8589
90	0·7853	0·7513
120	0·7109	0·6674
180	0·5444	0·5440
210	0·5022	0·5022
240	0·4703	0·4624
270	0·4317	0·4294
300	0·4068	0·4008

It follows, therefore, from the experiments the details of which are given above, that solutions containing both nitrous and nitric acids, whether prepared from silver nitrite and hydrochloric acid, or from nitric oxide and nitric acid, or from nitrogen peroxide and water, or from nitrous fumes and water, have this one property in common, that the diminution of reducing material, presumably nitrous acid, therein contained, proceeds in accordance with the same law. The velocity of the change seems, however, to be dependent rather upon the ratio of nitrous to nitric acid than upon the actual masses of either of them. But though these solutions have this one point in common, yet in other respects they differ most markedly. For bubbles of gas, namely nitric oxide, are rapidly given off from solutions prepared from nitrogen peroxide and nitrous fumes, and to a less degree from silver nitrite, though solutions from nitric oxide and nitric acid do not give off bubbles of gas in a similar manner. Again, all my former investigations* have shown that, whereas metallic lead does not readily dissolve in solutions containing nitrous acid, and prepared from silver nitrite, yet it dissolves very rapidly in solutions equally containing nitrous acid, but prepared from nitrogen peroxide and water. The cause for the similarity on the one hand, and the dissimilarity on the other, must remain the subject of a fuller enquiry.

General Conclusions.

The main points of this enquiry may briefly be summarised as follows:—

* 'Soc. Chem. Industry Jl.,' 1891, p. 1294.

(i.) The formation of the impurity of nitrogen peroxide in nitric acid, imparting to it the well-known yellow tint, takes place in the case of the more concentrated acid, even at a temperature of 30°, and of the less concentrated acids at from 100—150°, even when the acid is not unduly exposed to sunlight.

(ii.) The reaction between nitric oxide and nitric acid may be regarded as reversible, thus :—

$$2NO + HNO_3 + H_2O \rightleftarrows 3HNO_2,$$

provided that the acid be sufficiently dilute, and the temperature sufficiently low. Under these conditions equilibrium is established between the masses of nitric acids when the ratio of the former to the latter is, roughly speaking, as 9 : 1. The actual ratio varies slightly on the one side or the other, according to the conditions of the experiments. With more concentrated acids and at higher temperatures the chemical changes taking place are more complicated, and the decomposition of the acid more profound.

(iii.) The decomposition of solutions containing both nitric and nitrous acids is also investigated ; the rate of the change is shown to be proportional to the mass of the nitrous acid undergoing change. The curve representing the amount of chemical decomposition in terms of the mass present is shown to be hyperbolic, and illustrative of the law

$$\text{(I.)}\ \frac{dC}{dT} = -\frac{C^2}{M}.$$

This holds good, whatever be the method employed for the production of the nitrous-nitric acid solution.

The observed values for C or the concentration of the nitrous acid are concordant with those calculated according to the above differential equation within the limits of experimental error.

The rate is dependent upon the ratio of the masses of the nitrous and nitric acid, being the more rapid, the greater the proportion of the former to that of the latter.

In the particular case of the liquid prepared from nitric oxide and nitric acid, wherein the reproduction of solutions of similar concentration presents less difficulty, it is shown that as the temperature increases in arithmetical the rate of change increases in geometrical proportion, in accordance with the equation

$$\text{(II.)}\ v_t = v_{t_1} k^{(t-t_1)},$$

the value for k being 0·0158.

Finally, though the nitrous-nitric acid solutions behave in a similar manner as regards the diminution of the mass of the nitrous acid, yet, in other respects, such as evolution of gases and the action upon metals, they are dissimilar.

In conclusion, I would express my obligations to Mr. W. Esson for assistance in the mathematical portion of the paper, and to my colleagues for suggestions made in the course of the investigations.

VII. "On the Theory of Electrodynamics, as affected by the Nature of the Mechanical Stresses in Excited Dielectrics." By J. Larmor, M.A., Fellow of St. John's College, Cambridge. Communicated by Professor J. J. Thomson, F.R.S. Received April 25, 1892.

1. A theory of electrodynamics was first precisely developed by Maxwell, which based the phenomena on Faraday's view of the play of elasticity in a medium, instead of the conception of action at a distance, by means of which the mathematical laws had been primarily evolved. The electromotive equations of Maxwell however involve nothing directly of the elastic structure of this medium, which remains wholly in the background. They involve simply the assumption of a displacement across dielectrics with such properties as to make all electric currents circuital; all the equations of Ampère and Neumann for closed or circuital currents have then a universal validity, and no further hypothesis is required for the full development of the subject.

The theory was next discussed by Helmholtz in his memoirs on electrodynamics, in a way which took direct advantage of the picture of a polarised dielectric supplied by Mossotti's adaptation of the Poisson theory of induced magnetisation. Stated absolutely, this simply builds upon the assumption that at ecah point in the excited dielectric there is something which has the properties of a current element (electric transfer or displacement), which is represented both in direction and magnitude by the electric force at the point multiplied by a constant factor; no more general starting point seems possible for an isotropic dielectric. The development of this hypothesis, exactly on the analogy of a similar discussion with the Poisson-Mossotti phraseology in a previous paper,* leads to the necessity of recognising the existence of absolute electric charges on the faces of an excited condenser; so that the exciting current causes the accumulation of these charges, and therefore is not circuital or solenoidal. This defect of circuital character however practically disappears in the limiting case when the constant ratio of the polarisation to the electric force is extremely great; and then the theory becomes a concrete illustration of the general statements of Maxwell with respect to electric displacement.

* The Theory of Electrodynamics," 'Roy. Soc. Proc.,' 1891.

It was shown in the paper above referred to that this hypothesis, adopted by Helmholtz, led by itself—without any necessity for further assumptions which its author introduced on various grounds—to the undulatory propagation of electromotive disturbances across dielectric media, with the same transverse type of waves as constitute light. There will usually be, in addition, a disturbance of a *quasi*-compressional character, which, on the more special hypothesis of Helmholtz, is also propagated as a wave of permanent type, but with different velocity. The electric undulations of transverse type have been detected by Hertz; and the balance of evidence, from the experiments of different authors, seems to point to the conclusion that their velocities in different media are inversely as the square roots of the specific inductive capacities. Should this be fully verified, it would follow demonstratively that the Helmholtz hypothesis must be restricted to the special form which represents the Maxwell displacement theory; and the general equations of electrodynamics, or rather the electromotive part of them, will be definitely established.

2. The object here proposed is to pass on from the electromotive to the ponderomotive properties of the electric field, and examine whether the latter lend any strength to the conclusions derived from the former. Instead of a kinetic phenomenon like undulatory propagation, we shall now consider the static phenomena of the stress produced in the material of a dielectric by its excitation; and, to avoid the complexity, both optical and mechanical, introduced by the elasticity of solids, we shall consider solely liquid dielectrics, on which a very valuable series of experiments has been made by Quincke.* The mechanical stress in a fluid depends on one variable, the intensity of the hydrostatic pressure, and therefore may be connected immediately with the distribution of the energy in the medium, by means of the principle of work.

The arguments for the actual existence of a stress of the Maxwell type may be exhibited in a synthetical manner as follows:—Consider a condenser formed by two closed conducting sheets, one inside the other; and imagine the equipotential surfaces to be traced in the excited fluid dielectric between them. It is a matter of experimental knowledge that there is a traction on each face, acting inwards, and equal, at any rate approximately, to $KF^2/8\pi$ per unit surface, where F is the electric force. Now the electric potential and therefore the state of the dielectric fluid, will be in no wise altered if we imagine a very thin stratum along one of the equipotential closed surfaces to become conducting. There will therefore be a normal traction given by the same formula, on each element of area of this surface. If this traction is an affair transmitted across the medium, the transmitting stress must be a tension $KF^2/8\pi$ along the lines of force. To form an

* 'Wiedemann's Annalen,' vol. 19, 1883.

opinion as to whether a medium transmitting stress in this way could be imagined, let us suppose the dielectric divided into thin layers, like those of an onion, by much thinner conducting sheets, which coincide with the equipotential surfaces. The potential will not thereby be altered; if we run a tube of force across the dielectric, equal and opposite charges will reside on the portions of the two faces of each sheet intercepted by it. The layers of dielectric will be electrically independent of each other, being separated by conducting layers. Each dielectric layer will, therefore, form a condenser, and the energy of its electrification per unit surface will be $K(\delta V)^2/8\pi t$, or $KF^2t/8\pi$, where t is the thickness at the point, and δV the difference of potential between the faces; that is, there will be a distribution of energy $KF^2/8\pi$ per unit volume. The resultant traction on both the equal and opposite charges, each σ per unit area, on the two faces of a layer of dielectric, will be normal to the layer, and equal to $\frac{1}{2}\sigma(dF/dn)\delta n$ per unit surface; now, by Green's form of Laplace's equation, $\dfrac{dF}{dn} = F\left(\dfrac{1}{R_1}+\dfrac{1}{R_2}\right)$, where R_1, R_2 are the radii of principal curvature of the sheet; thus the traction is $\dfrac{F^2\delta n}{8\pi}\left(\dfrac{1}{R_1}+\dfrac{1}{R_2}\right)$. By the theorem of surface tension, this normal traction will produce and be balanced by a uniform surface tension along the sheet, of intensity $F^2\delta n/8\pi$, or $F^2/8\pi$ per unit thickness. In this laminated medium, owing to the attraction across the layers of very small thickness, we have thus set up a tension $KF^2/8\pi$ along the lines of force, which by reaction on the medium produces a pressure uniform in all directions round the lines of force, of the same numerical value. Or, again, we might, following Maxwell, postulate that the stress system in the medium must be symmetrical round the lines of force, and deduce, by the condition of internal equilibrium, that the tension and pressure of which it must thus consist are equal. A spherical system will form a simple illustration, capable of elementary treatment.

The fact that the surface of a dielectric liquid like petroleum is raised up by attraction, towards an electrified body brought near it, also affords evidence that this tension must exist. Consider two horizontal condenser plates, one inside the petroleum and the other over its surface in air. When the condenser is charged, the surface of the fluid rises between the two plates. There must, therefore, be some traction acting on it upwards to sustain it against gravity. The intensity of this traction is, in fact, according to Maxwell's law, $\dfrac{F^2}{8\pi}-\dfrac{K}{8\pi}\left(\dfrac{F^2}{K}\right)$, that is, $\dfrac{F^2}{8\pi}\left(1-\dfrac{1}{K}\right)$, where F is the electric force in the air; being positive, it acts upwards, in accordance with the actual phenomenon. Without the assistance of a traction of this kind, the fact would be unexplained, unless by assuming, with Helmholtz,

the existence of a *quasi*-magnetic polarisation of the elements of the medium; that would lead, on the interface between two media, to an uncompensated sheet of poles of density $\frac{K_1-1}{4\pi}F_1-\frac{K_2-1}{4\pi}F_2$, subject to a mean force $\frac{1}{2}(F_1+F_2)$; so that, as $K_1F_1=K_2F_2$, the traction would be $(F_1^2-F_2^2)/8\pi$, or, in the example chosen above, $\frac{F^2}{8\pi}\left(1-\frac{1}{K^2}\right)$. The discrepancy between these values might, perhaps, be amenable to experiment; but I find, on trial, that the difficulty of obtaining a clean unelectrified surface is not easily overcome.

The observation of Faraday, that short filaments of silk or other dielectric material suspended in a fluid dielectric set themselves along the lines of force when it is excited, is also evidence of actual internal polarisation related to the lines of force.

For the case of a fluid, the Faraday-Maxwell stress is made up of a hydrostatic pressure, $KF^2/8\pi$, which is consistent with simple fluidity, together with a tension $KF^2/4\pi$ along the lines of force, which requires for its maintenance qualities other than those of isotropic mechanical fluidity.

3. The polarisation theory, in the form of Mossotti and Helmholtz, which locates part of the electrification in a displacement existing in the elements of the dielectric, and part of it in an absolute electric charge situated on the plates of the condenser the cause of that displacement, is the representation of a wider theory which supposes the electrostatic energy to be in part distributed through the dielectric as a volume density of energy, and in part over the plates as a surface density. If experiment show that the latter part is null, we are precluded from imagining any superficial charge on the plates which has a separate existence, and is not merely the aspect at one end of the displacement across the volume of the dielectric. We shall find reason to conclude that there is no superficial part in the distribution of energy; this would carry the result that the excitation of a condenser consists in producing a displacement across the dielectric which just neutralises the charge conducted to the plates; it would also carry the result that all currents, whether in conductors or in dielectrics, must flow in complete circuits, and would therefore confirm the Maxwell theory of electrodynamics.

The conclusion that the location of all the electrostatic energy in the dielectrics involves that all currents flow in complete circuits seems of importance sufficient to justify a few remarks on the nature of the evidence on which it is based. The only precise notion or illustration of the nature of the dielectric polarisation which has yet been advanced is that of Poisson, which has been at various times used and developed by Mossotti, Faraday, Thomson, and Helmholtz. It might be held merely on general grounds that it gives a correct

formal view of the phenomenon, though the dynamical machinery of which it represents the action is quite unknown. But this presumption is very much strengthened by the fact that the displacement or polarisation is known to present qualitatively the properties of a true current, and also that the theory of dielectric propagation developed from this basis presents all the general analogies to light propagation that have been experimentally confirmed by Hertz and others. Taking it then that dielectric polarisation is formally of this type, the absence of a sensible absolute exciting charge on the bounding plates will show that it must be, so to speak, self-excited, that it is of the formal character of a displacement, of something pushed across from one plate towards the other like an incompressible substance.

4. Let us confine our attention for definiteness to the case of two metallic plates immersed horizontally near together in an extended mass of a fluid dielectric, so as to form a condenser. The traction T per unit area on the upper plate may by this arrangement be directly weighed. Suppose that there is a small aperture in the centre of the upper plate through which a volume of a different dielectric, say, a bubble of air, may be introduced between the plates, so as to form a flat cylinder co-axial with the plates and bounded above and below by them. The extra pressure P in this air bubble, when the condenser is excited, may be measured by a manometer in connexion with it, and it will give the means of determining the pressure in the surrounding liquid dielectric. This arrangement describes in fact the plan of Quincke's experiments.

At a point in the dielectric where the electric force is F, the electric pressure will be proportional to F^2, say, A_2F^2 for the liquid, and A_1F^2 for the air. The air column in the manometer tube would thus be in internal equilibrium with an electric pressure A_1F^2 next the liquid and null at the manometer end. We might at first glance infer that under these circumstances the pressure A_1F^2 is not indicated by the manometer, which would thus record simply the electric pressure in the liquid. But this air pressure is an internal stress; the equilibrium of any section of the air column requires, in order to maintain it, the electric pressure against it of the air on the other side of that section; therefore, the indication of the manometer really gives the differential effect $A_2F_2^2-A_1F_1^2$.

Let now the volume energy of the electrification be C_2F^2 in the liquid, and C_1F^2 in the air, each per unit volume; and let the energies of such real surface electric distributions as might exist on the plates in contact with these dielectrics be Σ_2 and Σ_1 respectively, each per unit surface of both plates. These surface energies would involve in their expression the electric potential as well as the force. We may apply the principle of virtual work to determine the relations between the quantities thus defined.

Suppose that the distance between the plates, with no air bubble introduced, is slowly increased from c to $c+\delta c$ against the tension T_2 which tends to draw them together. The work done against T_2 in raising the upper plate is the only source of the additional energy of the system which appears when that separation is effected. Now the value of F is not thereby altered, as the electrification remains constant. The volume energy contained in a cylinder of the liquid of unit sectional area is therefore changed from C_2F^2c to $C_2F^2c\,(1+\delta c/c)$. The corresponding surface energy is changed from Σ_2 to $\Sigma_2+\frac{d\Sigma_2}{dc}\delta c$. Therefore, by the principle of work, $T_2 = C_2F^2+\frac{d\Sigma_2}{dc}$.

Now suppose a large air bubble introduced between the plates; and suppose its volume to be increased by δv owing to a virtual displacement produced by pressing in more air. The virtual work $P\delta v$ so done must be equal to the increase of the internal energy of the system due to the displacement. This increase may be calculated from the energy function of the actual conformation, not of the displaced position, as internal equilibrium subsists; and all considerations as to change of intrinsic energy may thus be evaded.* For a bubble of small dimensions its surface would, in fact, be increased by the supposed displacement, and so there would be an increase of intrinsic capillary energy; with surface tension τ, radius a, and a semicircular meniscus πc, there would be an increase of amount $2\pi^2c\tau\delta a$, that is $\pi\tau c\delta v/a$. Again, the electric forces F_1 and F_2 in the two media, measured not close up to the meniscus, are equal, because the plates are each at uniform potential. Close to the centre of the meniscus they are both tangential to it, and must also be equal on the two sides. But along the slope of the meniscus they are oblique to it and are unequal, their relation being determined on the ordinary theory by the continuity of the normal component induction and of the tangential component force. Thus the difference of electric pressure across the meniscus will vary from point to point of it, and its form will, therefore, be slightly altered by the electrification. It follows from the observations of Quincke and others that its capillary constant will not thereby be altered, as was to be expected because there is no extra supply of molecular surface energy. There is also the alteration of intrinsic energy due to the fact that the expansion of the air bubble alters the electric force. But, according to the principles just stated, these changes of intrinsic energy balance each other, because all the parts of the medium are in internal equilibrium. We may therefore consider the annular mass between two ideal coaxial cylindric surfaces at a distance from the meniscus, one in the

* For applications of this principle, *cf.* Helmholtz, 'Wied. Ann.,' vol. 13, p. 388; and Kirchhoff, 'Wied. Ann.,' vol. 24, p. 57.

bubble and one in the liquid, and reckon the change in the energy contained in the space originally occupied by this annulus, when it receives a small displacement outwards from the axis under the action of the manometer pressure P. That change is $-(C_2-C_1)F^2\delta v$, where F is the electric force at a distance from the meniscus. Therefore, by the principle of virtual work, $P\delta v-(C_2-C_1)F^2\delta v = 0$, so that $P = (C_2-C_1)F^2$.

The value of the traction between the two plates in air is given by this formula as $T_1 = C_1F^2+\frac{d\Sigma_1}{dc}$; this must, therefore, be the same as the well-ascertained experimental value $F^2/8\pi$. Now the experiments of Quincke and others on liquid dielectrics have given reason to believe that, within the limits of experimental uncertainty due to want of purity of the materials and other causes, $T_2 = P+F^2/8\pi$. It follows that we must have $d\Sigma_2/dc = d\Sigma_1/dc$; that is, $d\Sigma/dc$ must be the same for all media, which is physically consistent only with the non-existence of this surface energy, unless we can suppose it to be the energy of an action at a distance which is independent of the intervening medium altogether.

The argument may also be expressed somewhat differently as follows:—The plates of the condenser being supported independently, the existence of an extra pressure on the dielectric when the condenser is excited shows that part, at any rate, of the electric energy resides in the dielectric. That part must, on any view, either of action by contact or of *quasi*-magnetic polarisation, be proportional to the square of the electric force at the point, which is in fact confirmed by the experiments of Silow on a quadrant electrometer with its needle immersed in a dielectric liquid filling the quadrants. If this were the whole of the electric energy, the traction between the plates would be equal to the hydrostatic pressure in the dielectric, or at most differ from it by an amount which would be the same for all media.* If this were only part of the electric energy, the difference would depend on the other superficial part. The experiments show that for a large number of liquids the difference is very nearly the same, so that if, after Quincke, we suppose it to be null for air or vacuum, it is null for all the others.† Hence either the superficial energy must be absolutely independent of the nature of the dielectric

* *Cf.* J. Hopkinson, 'Roy. Soc. Proc.,' 1886, p. 453, for the use of a similar argument in the converse manner, to show that the tension and pressure must be equal; but in it the energy of the polarisation of the medium is apparently not sufficiently traced.

† The results of Quincke are calculated so as to give values for K_p, the inductive capacity deduced from experiments on fluid pressure, and K_s, the inductive capacity deduced from experiments on the traction between the plates, on the assumption that the stress is of the Faraday-Maxwell type. The following examples show the order of magnitude of the discrepancies—

or else it must be non-existent. The precise logical statement of Quincke's results is, in fact, that the difference between the electric stress in a ponderable fluid dielectric K and the electric stress in a vacuum, in a field of force F, consists of a tension $\frac{K-1}{8\pi}F^2$ along the lines of force, combined with a pressure of equal amount in all directions at right angles to them; and this is consistent with a distribution of polarisation energy in the fluid, added to the electric energy for a vacuum with the same intensity of force, but not entering into combination with it.

Now the propagation of electrical waves across air or vacuum shows that even then, when there is no ponderable dielectric present, there must be a store of statical energy in the dielectric; and this fact appears to remove the only explanation which seems assignable for the division of the energy into two parts, one located in the dielectric, and the other located on the plates and absolutely independent of the dielectric, viz., that the latter might be the energy of a direct action across space which is not affected by the dielectric. The experimental facts, therefore, so far tend to the conclusion that at any rate the basis of electrical theory is to be laid on Maxwell's lines, with a reservation for possible modification in the form of residual corrections, but not for change of principle.

A theory has been developed by Helmholtz for fluids, and by Kirchhoff, following him, for solid dielectrics, in which slight residual differences between the intensities of the tension and pressure may be accounted for on the supposition that the inductive capacity, instead of being constant, is a function of the electric force. This theory is primarily expounded in terms of a polarisation scheme, and in so far is subject to the remarks of the next section; but it may in the end be based, as Helmholtz suggested, on the principle of energy applied with the aid of the ascertained form of the characteristic equation of the potential, treated as a condition of internal equilibrium. If we adopt the view that the difference to be explained has not certainly been detected,* this theory need not here be considered.

Some of the points in the general treatment given above will

	K_p.	K_s.
Ether	4·62	4·66
Carbon disulphide	2·69	2·75
Benzol	2·32	2·37
Turpentine	2·26	2·35
Petroleum	2·14	2·15

The chief difficulty seemed to be to avoid conduction, owing to want of purity of the dielectric fluid.

* *Cf.* Bos, "Inaugural Dissertation," abstracted in 'Philosophical Magazine,' February, 1891.

also be illustrated by the following brief discusion, which has special reference to the Mossotti-Helmholtz polarisation theory. In the course of it reasons will appear that even the special limit of that theory which coincides with Maxwell's as to form must be abandoned as inconsistent with the dynamical phenomena, in favour of a theory of pure contiguous action or strain of an incompressible æther.

Without entering here into detail as to the general characteristics of this kind of polarisation, it will suffice to point out some of its principal relations with regard to which misconception is easy, and also to point out the modifications which are necessitated in its usual form by the recognition of the discrete or molecular character of the polarised elements. In the Poisson theory of induced magnetism the magnetic potential is the potential not of the actual magnetism, but of the continuous volume and surface distributions of ideal magnetic matter which Poisson substitutes for it. The forces on a magnetic molecule are therefore not to be derived from it.* But if we imagine a very elongated cavity to be scooped out in the medium along the direction of magnetisation, and the molecule to be placed in the middle of the cavity, the forces of the remaining magnetised matter will be correctly derived from this potential. This part of the forcive will thus be derivable from a potential energy MF cos ϵ, where M is the moment of the molecule, F the resultant force derived from the magnetic potential, and ϵ the angle between their directions; we may thus consider a potential energy function IF cos ϵ per unit volume. We have to add to these forces the ones due to the rejected magnetic molecules which lay in the elongated cavity. Now the mutual action of contiguous magnetic molecules will be of the nature of a tension along the lines of magnetisation and a pressure at right angles to them, as Helmholtz remarked;† but these stresses will not necessarily be equal in intensity; nor will they represent the Faraday-Maxwell stress, since each component is proportional to the square of the coefficient of magnetisation, not to its first power. In a fluid medium these forces also must be derivable from an energy function, for otherwise the medium could not be in equilibrium; and the total potential energy per unit volume with its sign changed is equal to the fluid pressure. Thus in the polarised fluid the pressure is

$$\tfrac{1}{2}\text{FI}+\tfrac{1}{2}\lambda\text{I}^2,$$

that is,

$$\tfrac{1}{2}(\kappa+\lambda\kappa^2)\,\text{F}^2.$$

* In estimating these forces it is not allowable to replace the molecule by its three components parallel to the axes in the usual manner. This procedure would lead to error if there are electric currents in the field. *Cf.* Maxwell, 'Electricity,' ed. 2, vol. 2, ch. xi, appendix 2, p. 262.

† 'Wiedemann's Annalen,' vol. 13, 1881, p. 388.

An actual illustration in which the term involving λ is of predominant importance is afforded by a bunch of iron nails hanging end to end from a pole of a magnet; the adjacent nails hang on to each other lengthways and repel each other sideways, while the action of non-adjacent ones is but slight.

In the electric polarisation theory the specific inductive capacity is $K = 1 + 4\pi\kappa$. The results of Quincke, above mentioned, after they had been corrected for an experimental oversight in the direct determinations of the values of K by experiments on capacity, in accordance with a suggestion made by Hopkinson,* made the electric pressure to be $KF^2/8\pi$, consistently within the limits of experimental error for fifteen different substances. Thus, even in the limiting Maxwell form of the theory, which takes the absolute numerical value of K to be very great, this theory would not fit with the experiments unless λ is zero. Even by the purely mathematical device of taking the polarised elements to be right solids closely packed together, it does not seen possible to evade this argument.

In an actual fluid polarised in the above manner each element might on the average be considered as lying at the centre of a cavity, a sort of sphere of action within which the other molecules in their motions do not approach it further. On averaging the positions of these surrounding molecules during their motions with respect to the one under consideration, we arrive at the conception of a continuous polarised medium with a cavity in it of the form of this sphere of action. If this cavity were an actual sphere, the value of λ would be $\frac{4}{3}\pi$; and for cavities not very greatly different from the spherical form, the alteration in this value would be insensible. Under no likely circumstances could the value of λ come to be zero.

Thus the limiting Helmholtz polarisation representation of an excited dielectric, though complete as regards electromotive properties, would appear to fail to include the static ponderomotive phenomena of electrification, and requires to be modified into some more continuous mechanism, such as an elastic displacement in an æther loaded with the molecules of the dielectric.

It may be well to remark that, on account of the extreme smallness of the magnetic coefficient κ for all fluids, its square is of no account in comparison, and therefore magnetic pressures are sufficiently represented by the simpler formula $\frac{1}{2}\kappa F^2$, by means of which Quincke has measured the magnetic constants of various fluid media.

5. The principal conclusions which have been arrived at are here enumerated.

(i.) It is shown from experimental results that the stress in an excited fluid dielectric between two condenser plates consists, at any rate to a first approximation, of a tension along the lines of force and

* 'Roy. Soc. Proc.,' 1886.

an equal pressure in all directions at right angles to them, superposed upon such stress as would exist in a vacuum with the same value of the electric force.

(ii.) It is shown from experiments that the numerical value of these additional equal tensions and pressures is, at any rate to a first approximation, $(K-1)F^2/8\pi$, where F is the electric force, and K the inductive capacity.

(iii.) Such a distribution of equal tension and pressure is the result of a uniform volume distribution of energy in the dielectric, irrespective of what theory is adopted as to its mode of excitation.

(iv.) If we consider the mode of excitation to be a *quasi*-magnetic polarisation of its molecules, the numerical magnitude of these stresses should be

$$\frac{K-1}{8\pi}F^2\left(1+\lambda\frac{K-1}{4\pi}\right);$$

where λ is a coefficient which depends on the molecular discreteness of the medium, and is probably not very different from the value $\frac{4}{3}\pi$. A discrete polarisation of the molecules does not account for the stress, so far as this coefficient is concerned.

(v.) The stress which would exist in a vacuum dielectric is certainly due in part to a volume distribution of energy, as is shown by the propagation of electric waves across a vacuum. There is thus no reason left for assuming any part of it to be due to a distribution of energy on its two surfaces, acting directly at a distance on each other. There is therefore ground for assuming a purely volume distribution of energy in the vacuous space, leading to a tension $F^2/8\pi$ along the lines of force, and a pressure $F^2/8\pi$ at right angles to them.

(vi.) The *quasi*-magnetic polarisation theory rests on the notion of a dielectric excited by a surface charge on the plates, and therefore involves a surface distribution of energy, except in the extreme case when the absolute value of K is very great; in that case a slight surface charge produces a great polarisation effect, and in the limit the polarisation may be taken as self-excited. Thus the absence of a surface distribution of energy leads to Maxwell's displacement theory, in which all electric currents are circuital, and the equations of electrodynamics are therefore ascertained.

(vii.) It appears that even this limiting polarisation theory must be replaced, on account of the stress-formula in (iv), by some dynamical theory of displacement of a more continuous character.

6. We may perhaps attempt to form a more vivid picture of the interaction between æther and matter by following out the ideas of Lord Rayleigh's version of Young's theory of capillarity. We may conceive the compound medium, æther and matter, to consist of a very refined æthereal substratum, in which the molecular web of

matter is imbedded. The range of direct action between contiguous parts of the æther would be very small, and that between contiguous elements of matter large in comparison. There exist disturbances in which the matter-web is unaffected, its free periods being too slow to follow them: these are propagated with great velocity as light, or electrical radiations. There are other disturbances in which the matter-web is alone active; these are so slow that the æther can adjust itself to an equilibrium condition at each instant; they are propagated as waves of material vibration or sound waves.

When a dielectric is excited, we find ourselves in the presence of a strain of an æthereal origin somehow produced; it would relax on discharge of the system with the velocity of light. At an interface where one dielectric joins another, the æthereal conditions will somehow, owing to the nature of the connexion with the matter, only admit of a portion of the stress being transmitted across the interface; and there will thus be a residual traction on the interface which must, if equilibrium subsist, be supported by the matter-web, and be the origin of the stress which has been verified experimentally. Inside a conductor, the æther cannot sustain stress at all, so that the whole æthereal stress in the dielectric is supported by the surface of the matter-web of the conductor. At such interfaces the æthereal part of the distribution of energy in the medium will be discontinuous.

A formula has been given by Maxwell* for the intensity of the pressural force produced by electric undulations in the æther striking against a plate of conducting matter, a force which has apparently not been detected for the case of light-waves. If the notions here suggested have any basis, this force may likely be non-existent. For the pulsations of the æther at this surface may be so rapid as to prevent their energy being communicated to the matter-web of the conductor; and the energy will then be scattered and lost instead of appearing as energy of material stress. We may take as an illustration a stretched cord with equidistant equal masses strung on it, for which Lagrange showed that if the period of a disturbance imparted at one end exceeds a certain limit, the disturbance will not be transmitted into the cord, but will be eased off within a short distance of the point of application. And also in a manner which forms a more exact analogy, Sir G. Stokes has shown that the higher harmonics of a telegraph wire vibrating in the wind have their pulsations too rapid to get a grip on the air around them, and their note is therefore not transmitted.

This view would place the electrostatic and electrodynamic forces on matter on a lower plane, and in the case of rapid or sudden disturbance a more uncertain one, than the electromotive phenomena.

* 'Electricity,' § 793.

VIII. "On Current Curves." By Major R. L. HIPPISLEY, R.E. Communicated by Major MACMAHON, F.R.S. Received May 12, 1892.

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June 16, 1892.

The LORD KELVIN, D.C.L., LL.D., President, in the Chair.

Lieut.-Col. Robert Young Armstrong, Professor John Ambrose Fleming, Dr. Robert Giffen, Professor William Abbott Herdman, Mr. John Joly, Dr. Joseph Larmor, Professor Louis C. Miall, and Dr. Augustus D. Waller were admitted into the Society.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On a Multiple Induction Machine for producing High-Tension Electricity, and on some remarkable Results obtained with it." By the LORD ARMSTRONG, C.B., F.R.S. Received May 18, 1892.

[Publication deferred.]

II. "On certain Appearances of Beams of Light, seen as if emanating from Candle or Lamp Flames." By the late Professor JAMES THOMSON, F.R.S. Communicated by LORD KELVIN, P.R.S., with an Introductory Notice. Received June 10, 1892.

About the end of last January, when my brother was fully occupied in writing his paper on the Trade Winds for the Bakerian Lecture, he called my attention to the well-known beams or ladders of light seen below or above a lamp flame viewed with partially-closed eyelids, and he gave me verbally an explanation of the phenomenon which surprised me very much. By some simple and interesting trials with my own eyes, which he explained to me how to make, I was perfectly convinced that his explanation was correct; and believing it, as I still believe it, to be new, I urged him to write a short paper on the subject for the Royal Society, but not to let it interfere with his work for the Bakerian Lecture; and he undertook to do so as soon as might be after being freed from this work. We hoped, somewhat confidently, that he might be able to give the thus promised paper before the end of the present session of the Royal Society. That

hope has not been fulfilled, and I had offered to the Secretaries a communication describing my recollection of what my brother had told me, when his son found a memorandum of date 18th October, 1891, and a little book of notes of date 29th December, which tell the story better than I could have told it, and which, therefore, though not completed in proper form for publication, I now give in the unfinished form in which they have been found, with only a somewhat more clear drawing, and description of drawing, substituted for the rough sketch found in his note of date October 18, 1891.

Proposed probable Paper for the (?) Society, by J. T., "On the Nature and Origin of certain Appearances of Beams of Light as if emanating from Candle or Lamp Flames."

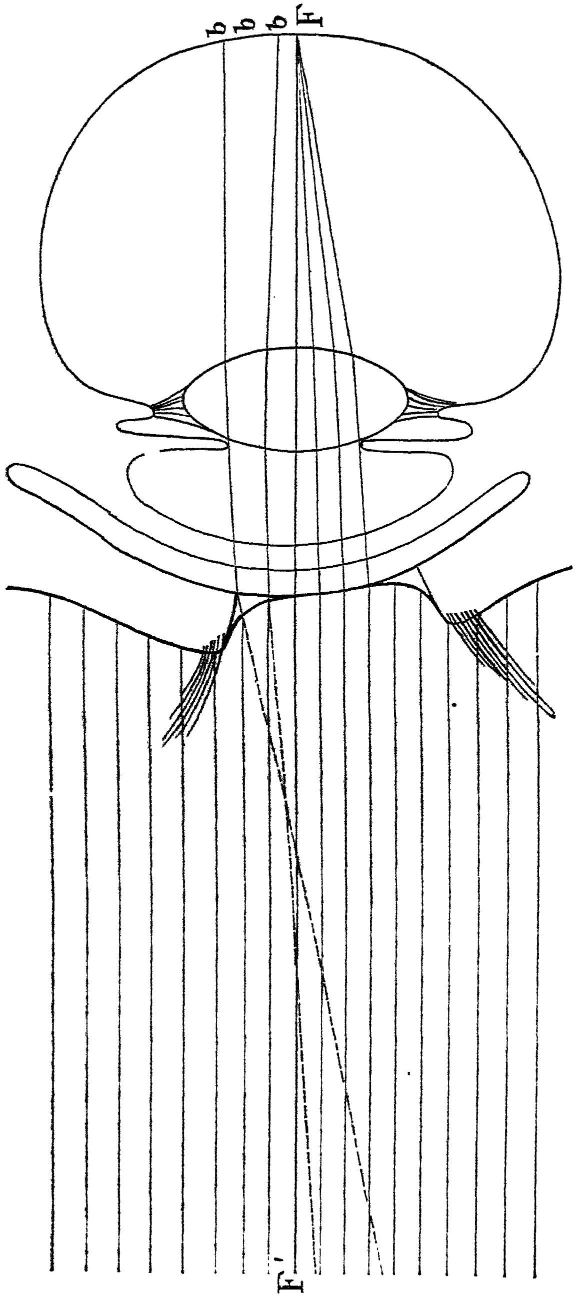
Description of the Drawing.

[The drawing represents a vertical section of the eye, eyelids, and watery prismoids,* through FF′, the axis of the eye. The large number of parallel lines outside represent rays of light coming from a flame several feet or yards away in the direction of F′, to the eyelids, the prismoids, and the undisturbed outer surface of the cornea between the prismoids. The lines within the eye below FF′ represent the convergence to F, the image of the flame, of those of the external rays from the flame which fall on the undisturbed portion of the surface of the cornea. The lines within the eye above FF′ represent rays disturbed by the prismoid of the upper eyelid which, incident on the retina at *bbb*, give the perception as if of light coming from without in the direction of the dotted lines outside the eye. It is this perception that constitutes the appearance of the downward beams or ladders of light, due to the prismoid of the upper eyelid. The rays disturbed by the prismoid of the lower eyelid, in the position represented in the diagram, are all stopped by the lower part of the iris.

Looking now at the diagram, we understand perfectly that if, with the eyeball and flame unchanged, the upper eyelid be gradually raised a little, the uppermost of the rays coming inwards from the prismoid will fall on the upper part of the iris and will be stopped by this screen. Thus, the length of *bbb* upwards from F is diminished, until all the beams from the prismoid are stopped by the iris, and the length of the apparent beams below the flame correspondingly diminishes to zero. When the upper eyelid is wide open the flame is seen without any appearance of the beams below it. We also understand readily from the diagram how, if the lower eyelid is lifted a little without any change in the position of the upper eyelid, beams both above and below the flame are seen. We also conclude that if,

* The refracting watery liquid in the entrant corner between lip of eyelid and cornea may be called the prismoid or liquid prismoid.

Rough Sketch (imperfect).



with the eyelids fixed relatively to the head, the head is moved while the eyeball remains with its axis in the direction of the flame, we see beams of light above the flame when the head is turned upwards, and beams of light below the flame when the head is turned downwards. Also that if the eyelids are partially closed, as in the diagram, beams will be seen both above and below the flame when the head carrying the eyelids with it, is turned slightly up from the position shown in the diagram. Also that if the eyelids be wide open, instead of half closed as shown in the diagram, no beams, either above or below the flame, will be seen when the two eyelids are equidistant, or nearly equidistant, above and below the middle of the pupil. When the head, with the eyelids, is turned downwards, so as to bring the upper eyelid across the aperture of the pupil, beams of light are seen below the flame; and when the head, with the eyelids, is turned upwards so as to bring the lower eyelid across the middle of the pupil, beams of light produced by the prismoid of the lower eyelid are seen above the flame.]

Notes on Quasi-Ray Beams of Light from Candles, or other small Luminous Spots.

Date of Note, 29th December, 1891.

I have noticed decidedly this morning to the following effect:—

In some cases (the nature of which I intend to note further on) I found that, when seeing a small gas flame with apparent descending tail (or quasi-beam of rays), I could, by lowering the upper eyelid, cut off vision of the flame, while leaving the tail visible; and, by still further lowering the upper eyelid, I could cut off the upper part of the tail, leaving the lower part, the part remote from the flame, quite visible as before. The contrast between lowering the upper eyelid and lowering a screen (a card, for instance) in front of the eye was very remarkable. In the lowering of the card or other screen, the tail vanishes before the flame is eclipsed; but in lowering the eyelid the flame is eclipsed first.

In some attitudes I could not bring out these phenomena. I did find them when awake in bed early in the morning, head on pillow and light coming down from a gas flame obliquely to the eye. Point to which eye was directed seemed to do best when taken at an altitude (angular) somewhat above the gas flame.

Afterwards, this same morning, I found I could see the phenomenon when standing upright and looking at image of gas in mirror. Ray from image ascending obliquely; eyesight directed above image in looking-glass.

Again, looking at a gas flame a little above the level of the eye, I stood erect and elevated my face, directing my eyesight to above the

gas; then lowered the upper eyelid and saw the downward tail remaining when the gas flame was eclipsed by the eyelid. The theory of all this is clear to me, and in agreement with what I have previously devised.—J. T.

Take notice that to get the phenomena above sketched out to show themselves, the edge of upper eyelid, where roots of eyelashes are situated, must not shadow the prismoid when the eyelid is lowered enough to cover the pupil from the direct rays of the candle or gas flame. After the candle is cut off from the pupil, the direct rays from the flame must still be reaching the prismoid. This, I think, tallies with the experimental conditions under which the tail was seen when the flame was eclipsed by eyelid.—J. T.

P.S.—Same day, 29th December. On a little further consideration I notice that the elevation of the face is of no importance. It is only the elevation of the line of special direction of the eyesight [axis of the eye] relatively to the line from flame to eye that is important.—J. T.

Notes on Quasi-Light Beams.

(For paper.)

Often I fail to see the apparently ascending beam above the candle or gas flame. But I find that by very nearly shutting the eye I can see the ascending beam going up very high and the descending one at same time. On bringing my open hand down from above as if to cut off the ascending beam I see the beam as if between my eye and my hand, and the flame begins to be eclipsed before the beam is cut off, or even diminished.

Note by the President of date June 16.

I had asked many friends well acquainted with optical subjects whether they knew of this explanation of the luminous beams, and all said no until yesterday evening, at the *soirée* of the Royal Society, when Professor Silvanus Thompson immediately answered by giving the explanation himself, and telling me that he had given it to his pupils in his lectures on optics, as an illustration of a concave cylindrical lens. He did not know of the explanation ever having been published otherwise than in his lectures. I have myself also looked in many standard books on optics, and could find no trace of intelligence on the subject. It seems quite probable, therefore, that, of all the millions of millions of men that have seen the phenomenon, none, within our three thousand years of scientific history, had ever thought of the true explanation except Professor Silvanus Thompson and my brother.

III. "Voltaic Cells with Fused Electrolytes." By J. BROWN. Communicated by Professor EVERETT, F.R.S. Received May 27, 1892.

In attempts to compare the observed electromotive forces of voltaic combinations with the theoretical values found by Thomson's* law, the discrepancies observed in many cases, and the difficulties experienced in ascertaining their nature and origin, are well known. It is evident that, in order to compare observations with theory, we must know exactly the facts on which the theory is to be founded, *i.e.*, the true nature of the chemical reaction in the cell, and its relation to the electric phenomena to be observed; and for this purpose simplicity in the construction of the cell is, in the present state of knowledge, almost a necessity. In many of the cells that have been examined heretofore, this condition of simplicity is not realised; and the nature of the chemical action in them is not definitely ascertainable because of the complicated nature of the substances employed. I have referred to this point in the introduction to a paper on the "Rôle of the Cation in Voltaic Combinations;"† and in a foot-note on p. 462, of that paper I have again referred to what is a very frequent cause of uncertainty, viz., the action of the solvent—usually of course water—as distinct from the dissolved electrolysable substances. This uncertainty is still more marked in the case of some of Wright and Thompson's experiments,‡ where insoluble salts suspended in solutions were supposed to be alone active to the exclusion of both the water and the dissolved substances. The solvent may act chemically on the metals, and affect the electromotive force, either directly as an electrolyte, or indirectly by the formation of films insoluble in itself, which act differently from the metals or protect them from chemical action. Laurie§ and Braun‖ have respectively shown that the lowering of the forces of aluminium and magnesium in aqueous solutions is probably due to such protective films; Julius M. Werner¶ considers the cause of the comparative inaction of pure zinc in acids to be that, at the moment of immersion, it becomes enclosed in a continuous film of hydrogen which protects it from further action; while with impure zinc the hydrogen forms on the impurities only, leaving the zinc exposed.

* 'Phil. Mag.,' ser. 4, vol. 2, p. 434, 1851.
† 'Phil. Mag.,' ser. 5, vol. 31, p. 449, 1891.
‡ 'Phil. Mag.,' ser. 5, vol. 19, p. 211, 1885.
§ 'Phil. Mag.,' ser. 5, vol. 22, p. 213, 1886.
‖ 'Phil. Mag.,' ser. 5, vol. 27, p. 209, 1889.
¶ 'Ber. Deut. Chem. Gesell.,' vol. 24, p. 1785.

The probability of such actions does not, I think, usually receive sufficient consideration in electrochemical investigations.

Again there is, in connection with the solvent, the uncertainty whether, in calculating the mechanical equivalent of the action in the cell, we should include the heats of solution of substances formed by the action. Where the product is easily soluble, the heat of solution is probably adjuvant; where insoluble, there is of course no heat of solution adjuvant; where it is soluble with difficulty, the heat of solution may be partly or to some unknown extent adjuvant.

The consideration of these difficulties, and especially the complication probably caused by the action of the water on magnesium and aluminium, as mentioned above, led me to the consideration of a much simpler form of cell, in which the solvent with all its complications is abolished, and the simple electrolyte in the fused state used alone.

The number of possible combinations of this kind is limited by the considerations that the electrolytes must be fusible at reasonable temperatures; and that the boiling point of any component in a cell must not be below the fusing point of the electrolyte in contact with it. A few of the haloid compounds of the metals are suitable; and the chlorides were chosen to begin with, as in some respects best known.

The results obtained show at all events the direction in which to look for an exact accordance between theory and experiment.

I. *Cells with Simple Chlorides.*

The cells were of course of the two-fluid type, each metal being in contact with its own chloride only, while the two chlorides were in good contact with each other. The type may be represented for instance as

$$\text{Zinc} \mid ZnCl_2 \mid MgCl_2 \mid \text{Magnesium}.$$

We cannot say the fused electrolyte is always free from the complication of dissolved oxygen, for in some cases this makes itself evident by the formation of oxychlorides. This very formation of oxychlorides may, however, remove the oxygen from the sphere of action.

We must admit also that in some of the combinations tried, action goes on with open circuit, since several of the metals, notably iron and copper, were somewhat corroded after immersion in the fused salt.

Further, while the difficulty about the heat of solution of the nascent salts is avoided, there is a question as to whether the heat of solution of one fused chloride in the other, *i.e.*, the heat of combina-

tion of the two chlorides with each other affects the electromotive force of the cell. It is impossible to answer this question without further data of various sorts. The small irregular variations from theory shown in the tables may possibly be due to some action of this kind. The results of experiment show, however, that these few complications have but a small effect; very much less than those of the aqueous form of cell with its metals coated with oxide and hydrogen films, and its mixture of two or more electrolytes and dissolved oxygen, such as might perhaps be represented as

$$\text{Zinc} + ZnO + H \mid ZnCl_2 + H_2O + O \mid MgCl_2 + H_2O + O \mid \text{Magnesium} + MgH_2O_2 + H,$$

which seems to be too complicated for exact calculation.

In arranging the practical details, care was taken to keep the metals as clean as possible, the contacts good, the electrolytes fused but not boiling, and the temperatures fairly equal at the two metals; also to prevent, as far as possible, the two chlorides from diffusing into one another. After trying various arrangements, that of which fig. 1 represents about half the actual size was adopted.

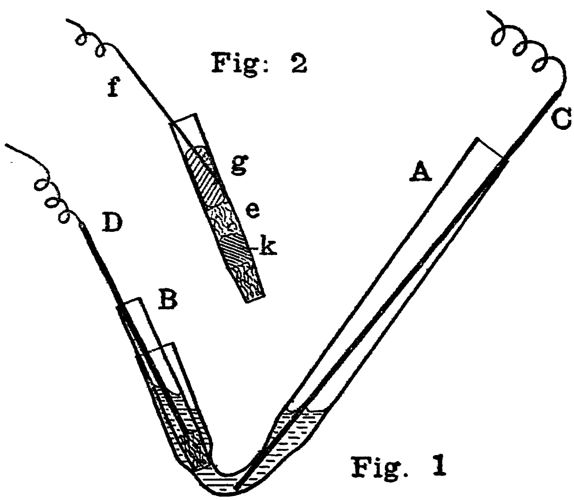


Fig: 2

Fig. 1

A V-tube A of hard glass, formed as shown, and supported by its longer limb, contained one of the chlorides, in which dipped the wire or strip of appropriate metal C. The bend of this tube was heated as required by one or two Bunsen burners. Into the shorter limb was inserted the other tube B, its lower end contracted somewhat, and filled with a plug of asbestos, to act as a porous partition. This smaller tube contained the second chloride, with its appropriate metal D. When the metal represented by C was fusible at nearly

the same temperature as its chloride, it was first fused in the bend of the tube A, and then its chloride fused on it in the shorter limb. The melted metal was connected to the Thomson electrometer by a wire of either copper or iron dipping in it. When this wire was of copper, long contact with the fused metal seemed to dissolve it somewhat; and a slight error may thereby be introduced in the cases of tin, zinc, and cadmium; but this is probably not large in any case. For these fusible metals, a clay tobacco pipe was sometimes substituted for the tube A, the connecting wire passing through its stem. When the metal in the small tube was of this easily fusible kind, it was found best to add another asbestos plug, as at *e*, fig. 2, which was permeated by the chloride below it at *k*, and supported the fused metal *g* with its connecting wire *f*.

While all mixture of the two chlorides was to be avoided as far as possible, it was considered especially necessary to keep the chloride of higher combining heat, which to save repetition we may call H, from being permeated by that of lower combining heat L, for the obvious reason that, when L mixed with H, its metal became at once reduced on the metal in H, and the conditions were thereby altered. If, for example, in the cell

Copper | Cu_2Cl_2 | $ZnCl_2$ | Zinc

the copper chloride (L) diffuses into the zinc chloride (H), copper becomes reduced on the zinc, and the electromotive force is lowered.

To obviate or mitigate this difficulty, the following precautions were taken:—The V-tube A was first set up, and the chloride fused in it. When the arrangement was such that chloride H was in the small tube B, this tube was first held in the Bunsen flame till the chloride fused and saturated the porous plug to the end or nearly so. The tube B was then inserted in its place, any small mixture of H with L not being considered of importance. If, on the other hand, H was in A, and L in B, then B was simply heated till L was about fusing point, and was immediately inserted in A, so that H had time to soak up into the plug before L soaked down.

The following was the usual plan of observing the electromotive force:—As soon as the electrolytes were in order, and the metals, previously connected to the electrometer, were inserted, the deflexion was noted immediately, and again after about five minutes. The metals were then, if solid, taken out, cleaned, and re-inserted, and the observations repeated. In these sets of observations the variation from the mean of any given experiment was less with fused metals than with solid metals, the latter being more liable to surface alterations. The adopted mean of each set was derived from those experiments in which the theoretical conditions were most strictly fulfilled.

As regards the quality of the substance used, the zinc, lead, iron, tin, and copper, and the chlorides of magnesium and tin, were purchased as pure. The other metals and chlorides were the substances as supplied by the chemical manufacturers under their respective labels. In the case of metals forming more than one chloride, that one was chosen in which a chlorine molecule Cl_2 gave the highest heat of combination. The corresponding formulæ are stated in the tables of results. The heats of combination employed are Thomsen's.

Table I gives the results obtained with the chlorides of the metals as stated, together with the theoretical values; the unit employed being the electromotive force of a Daniell cell, which for the Daniell I used may be taken as about 1·13.

In calculating the theoretical value, it is assumed that the reaction is simply the substitution, in the double fluid electrolyte, of the metal with higher combining heat for that with lower; and, therefore, that the theoretical electromotive force is, according to Thomson's law, simply proportional to the difference of their combining heats with chlorine, which is the anion of the electrolyte. This difference, divided by 50,130, the heat equivalent of a Daniell cell, gives the theoretical electromotive force for each pair, in terms of the Daniell. In practice, it is more convenient to divide, once for all, the heat of combination of each metal by the heat equivalent of the Daniell. The quotients thus found are placed under their respective metals in the table. It seems possible that these numbers may represent the true differences of potential at contact of metal and liquid.

The theoretical electromotive force for any pair of metals is then given by the difference of their respective numbers. In the table, this will be found in heavy type, at the intersection of the vertical and horizontal lines passing through any given pair of metals; the metal to the right forming the negative pole of the arrangement. Immediately below each theoretical electromotive force, is placed that found by experiment, being the mean of the number of sets of experiments indicated by the figure in brackets below it. The highest and lowest observed values are stated to the right and left of it.

In this table it will be observed that, while the mean experimental result for any combination of the metals zinc, lead, and tin agrees fairly well with Thomson's law; those combinations in which silver, copper, or cadmium occur do not exhibit such good agreement; and closer examination shows that much more consistent results could be obtained by applying a constant correction for each metal. I have determined, by a tentative process, the best values for these corrections, and the results are shown in Table II. It is only a question of differences; and if the correction for cadmium be taken as zero the corrections for all the other metals will be positive. A justification for this selection of cadmium will be found in a later paragraph.

Table I.

					Zinc in $ZnCl_2$. **1·939**
				Cadmium in $CdCl_2$. **1·86**	**0·079** 0·12, 0·13, 0·13 (3)
			Lead in $PbCl_2$. **1·651**	**0·209** 0·11, 0·14, 0·18 (4)	**0·288** 0·17, 0·22, 0·25 (6)
		Tin in $SnCl_2$. **1·612**	**0·039** 0·05, 0·05, 0·06 (3)	**0·248** 0·15, 0·16, 0·17 (3)	**0·327** 0·28, 0·31, 0·35 (5)
	Copper in Cu_2Cl_2. **1·812**	**0·3** 0·14, 0·16, 0·19 (2)	**0·339** 0·18, 0·21, 0·24 (5)	**0·548** 0·26, 0·31, 0·36 (4)	**0·627** 0·40, 0·46, 0·51 (4)
Silver in Ag_2Cl_2. **1·172**	**0·14** 0·08, 0·10, 0·11 (2)	**0·44** 0·28, 0·30, 0·32 (2)	**0·479** 0·30, 0·31, 0·32 (3)	**0·688** 0·37, 0·39, 0·41 (3)	**0·767** 0·51, 0·52, 0·53 (2)

Table II.—Table I modified.

					Zinc in $ZnCl_2$.
					1·939 + 0·049 = 1·988
				Cadmium in $CdCl_2$.	
				1·86	**0·128**
					0·13
			Lead in $PbCl_2$.		
			1·651 + 0·099 = 1·750	**0·110**	**0·238**
				0·14	0·22
		Tin in $SnCl_2$.			
		1·612 + 0·093 = 1·705	**0·045**	**0·155**	**0·283**
			0·05	0·16	0·31
	Copper in Cu_2Cl_2.				
	1·312 + 0·23 = 1·542	**0·163**	**0·208**	**0·318**	**0·446**
		0·16	0·21	0·31	0·46
Silver in Ag_2Cl_2.					
1·172 + 0·273 = 1·445	**0·097**	**0·260**	**0·305**	**0·415**	**0·543**
	0·10	0·30	0·31	0·39	0·52

Table II shows these corrections applied to each metal, and a comparison of these results with the mean experimental results, the order of arrangement being the same as in Table I. It will be seen that, with two exceptions, lead-cadmium 0·03, silver-tin 0·04, the discrepancy is always below 0·03.

This is interesting so far as it goes; and the next step is to inquire if a physical cause can be assigned for these constants. That they have their chief origin in the high temperature at which the experiments were made seems very probable. It has been shown by Gladstone and Tribe* that in the cells

Silver | Ag_2Cl_2 | Silver

Copper | Cu_2Cl_2 | Copper

if one of the metal-electrolyte junctions be heated more than the other, a difference of potential between the two metals is established, and a current may be produced from hot to cold through the electrolyte. I have made similar cells, using V-tubes containing the fused salts, with wires of the appropriate metals in each limb. In the copper cell, with one junction a little above the fusing point of the double chloride Cu_2Cl_2KCl (whose low fusing point allows a greater difference of temperature to be employed), and the other junction at a red heat, the observed electromotive force was about 0·1 Daniell, and gave a strong current through a low resistance galvanoscope, which could be reversed very prettily by simply moving the Bunsen

* 'Phil. Mag.,' ser. 5., vol. 11, p. 508, 1881.

lamp from one side of the cell to the other. A silver chloride cell of the same form gave a similar result. Lead was tested in the fused state in two asbestos-plugged tubes, like B, fig. 1, immersed in a V-tube containing fused lead chloride. The other easily fusible metals, tin, cadmium, and zinc, were tested in W-shaped tubes, open at the apex in the middle, for introduction of the chloride after the metal had been fused in the two lower bends. With all the easily fusible metals, except zinc, the tubes were made sufficiently long to allow the portion of metal surrounding the copper connecting wire to remain solid, so as to avoid risk of alloying from long fusion in contact with the copper.

In all cases, except cadmium, with which in three carefully-made experiments, I found no decided effect, the hotter metal was found to be the negative pole of the arrangement. Zinc was somewhat doubtful, and may have been influenced by alloying with the copper connection.

To give some idea of the magnitudes of the electromotive forces of these thermal cells, the results of these rough experiments are given in column A below. In column B are the corrections employed in Table II for comparison.

	A.		B.
Silver..........	0·07	Daniell	0·273
Copper	0·10	„	0·23
Tin............	0·01	„	0·093
Lead...........	0·015	„	0·099
Cadmium	0·00	„	0·00
Zinc	0·02 ?	„	0·049

There is a kind of rough correspondence between the numbers in the two columns, but the inexact nature of the experiments of column A precludes a strict comparison.

I had no means of measuring, otherwise than by rough estimation, the difference of temperature between the two sides of the cells; but as a mere approximation it might be taken for tin as rather less than the difference between the freezing and boiling points of stannous chloride, or say 300° C.; for zinc and copper it was rather more than this, and for lead and silver less, as their chlorides are less easily fusible, and the higher temperature was limited to the softening point of the glass tube.

Besides the experiments of Gladstone and Tribe mentioned above, others bearing less definitely on the subject have been made by Andrews,* Hankel,† and more recently by Mr. T. Andrews,‡ of Sheffield.

* 'Phil. Mag.,' ser. 3, vol. 10, p. 433, 1837.

† 'Pogg. Ann.,' vol. 103, p. 612, 1858.

‡ 'Roy. Soc. Proc.,' vol. 38, p. 216, 1885.

As regards these currents due to difference of temperatures, Andrews concluded, from the non-corrosion of the platinum wires used by him, that the action was in his experiments thermo-electric. Gladstone and Tribe state that in their experiments "it is difficult to imagine that chemical action in any way initiates the current." Their experiments show clearly, however, that the phenomenon is of electrolytic nature, since the current formed is accompanied by the solution of metal at the hot side, and its deposit in crystalline form at the cooler electrode. The same effect was also observed in my experiments with copper in its fused double chloride; a large deposit of crystalline metal was found on the cooler wire after the current had passed for an hour. The action must, therefore, be of electrolytic or voltaic character; and the application of Thomson's law to these unequally heated cells would suggest that, with the apparent exception of cadmium, there is an increase with temperature of the heats of combination of the metals with chlorine, conjoined with a possible Peltier effect. Such a variation is, in fact, well ascertained in other cases; and Thomsen* gives a formula for its calculation from the specific heats of the bodies involved. The want of data for the fused salts prevents its further discussion here.

Assuming this variation of the heats of combination to exist, and assuming, also, that the chemical energy is all adjuvant at the various temperatures,† we could directly account for the additive corrections empirically deduced from Table I and applied in Table II, the numbers in Table I being founded on Thomsen's heats of combination obtained at 18° to 20°, while my experiments were made at temperatures several hundred degrees higher. It will thus be important to determine the temperature co-efficients of these cells, and I hope to do so at some future time.

Another point demanding careful attention is the extent to which these cells are reversible. The following experiments bear on this subject:—

Two clay tobacco pipes were clipped by their stems in wooden supports, one in each, and placed with their bowls touching and facing upwards. One contained some melted zinc, and the other melted tin, with iron connecting wires through the stems. The respective chlorides were fused over these metals and connected together by a bunch of asbestos, a notch having been cut at the point

* 'Thermochem. Untersuchungen,' vol. 2, p. 54.

† [This assumption is of course provisional. It was necessary because of the absence of any investigation here as to what part, if any, of the energy is non-adjuvant. Helmholtz and other well-known authors have discussed this question in connexion with the ordinary aqueous form of cell, but I have not attempted its investigation as yet in the present case. When the temperature coefficients and other data have been obtained this may be undertaken.—*Aug.* 2, 1892.]

of contact of the bowls to receive this. The bowls were covered by a porcelain lid. The cell in this form gave a deflexion on the electrometer of 43 divisions. It was then subjected to the following consecutive tests. The entries are to be read consecutively from left to right, the first observation in the second column being immediately subsequent to the last observation in the first column, and so on. "Short circuited" means that the current of the cell was passed through a low resistance astatic galvanoscope, sending the needle to the stops; "reversed" means that a nearly equal current, from a Daniell cell, was sent in the opposite direction through the cell under examination and the galvanoscope. After the duration of either of these currents for the times given below, the force of the cell was again measured on the electrometer, with the results stated :—

Zinc | $ZnCl_2$ | $SnCl_2$ | Tin.

				Means.
Short circuited	15 secs.	30 secs.	5 min.	
Deflections	41	40·5	42	41·2
Reversed	30 secs.	30 secs.	5 min.	
Deflections	41	41	42	41·3

A similarly arranged zinc-lead cell gave, when set up, a deflection of 22, and then the following deflections were noted, immediately after the cell had been either short circuited, reversed, or rested on open circuit, for a period of four minutes in each case, except the first two observations, which were for one minute each.

Zinc | $ZnCl_2$ | $PbCl_2$ | Lead.

					Means.
Short circuited....	22·5	20·0	18·0	18·5	19·75
Reversed	20·5	18·5	18·5	18·0	18·87
Rested	..	18·0	17·5	18·0	18·87

Zinc-silver and zinc-copper cells were tested in the form adopted for the general investigation. The silver chloride was in the tube B, fig. 1, with an asbestos plug 3 cm. long, to prevent mixing of the chlorides in the separate tubes during the experiment. The copper chloride was in a similar tube, corked to exclude air. The durations of short circuit, rest, and reversal were each four minutes, except in the second and third observations with the zinc-copper cell, in which they were each one minute.

Zinc | $ZnCl_2$ | Ag_2Cl_2 | Silver.

								Means.
Short circuited..	85·5	85	85·5	—	84	78·5	—	83·7
Reversed	87·5	87	—	87	87	—	81	85·8
Rested	86·0	—	85·5	86	84	81·0	—	84·6

Zinc | $ZnCl_2$ | $CuCl_2$ | Copper.

							Means.
Short circuited......	—	49	55	44	46·5	—	48·6
Reversed...........	—	46	46	41	—	50	45·8
Rested.............	48	—	44	43	49·0	—	46·0

Except in the zinc-silver cell, and to a minute extent in the zinc-tin cell, there is no evidence of polarisation by reversal. The other two cells show the remarkable property of possessing a greater electromotive force after short circuiting than before it; and since this recurs repeatedly in the observations on both cells, it would seem to be more than accidental.

II. *Cells with Double Chlorides.*

As I have already stated, the present investigation was suggested by the behaviour of magnesium and aluminium in aqueous electrolytes; and as a matter of fact these metals were experimented on in the more complicated cells described below before the simpler cells of Table I were tried. Owing to the high fusing point of magnesium chloride, and the volatility of the aluminium compound, it was inconvenient to work with these chlorides in the fused state; and this difficulty had to be surmounted by employing the double chlorides of magnesium and potassium, and of aluminium and sodium. A similar method was adopted with ferrous chloride, which is infusible by itself.

It may be objected that this method introduces a solvent, the very complication which it is desired to avoid, since potassium chloride may be looked upon as the solvent of the other chlorides. But in comparing fused potassium chloride with water as a solvent of chlorides, there are two important differences. (i) With potassium chloride, the anion is the same as that of the dissolved substance, while with water it is not. (ii) The heat of combination K, Cl is greater than that of any of the other chlorides X, Cl dissolved in it in these experiments; therefore the metal X when immersed

in the double salt does not decompose the potassium chloride, but only reacts with its own chloride. Hence we have grounds for assuming that potassium chloride remains practically inert, so far as concerns any direct chemical action between it and the metal immersed. The potassium chloride is, however, active in another way, viz., in combining with the various metallic chlorides to form their double salts; and the heat of this combination should no doubt be taken into account. Unfortunately, the heats of formation of these double salts appear to be unknown. If they are nearly equal, so that their differences are small in comparison with the whole heat equivalent of the cell, their effect will be negligible. A comparison of Tables I and II shows that this is probably so, but naturally the results of these experiments with double salts cannot be compared with theory quite so satisfactorily as those with simple chlorides.

In any given experiment the two chlorides employed should be either both double or both single, to avoid the complication which would otherwise be introduced by the formation of a double salt on one side and not on the other. In the preliminary stage of this work, some experiments were made without observance of this rule, and the discordant results obtained showed that it cannot be disregarded. It was therefore necessary to form double chlorides for all the metals employed; and the combinations whose formulæ appear in Table III were found, either by reference to the books or by experiment, to be suitable.

The experiments were carried out in the same way as the first set, and the results are given in Table III, where for magnesium, iron, and aluminium, they are stated in the same form as in Table I. For the other metals, only single observations, as a rule, were made, for comparison with Table I. The corresponding numbers in both tables are at least of the same order of magnitude, the difference only in one case appearing in the first decimal place.

Table III.

								Magnesium in $MgCl_2KCl$. **3·012**
							Aluminium in $Al_2Cl_6Na_2Cl_2$. **2·141**	**0·871** 0·78, 0·75, 0·64
						Zinc in $ZnCl_2K_2Cl_2$. **1·939**	**0·202** 0·14, 0·17, 0·22 (4)	**1·073** 0·80, 0·87, 0·90 (3)
					Cadmium in Cd_2Cl_4KCl. **1·86**	**0·079** 0·19	**0·281** 0·40, 0·44, 0·48 (4)	**1·152** 1·04, 1·17, 1·21 (3)
				Lead in $PbCl_2K_2Cl_2$. **1·651**	**0·209** (0·13)	**0·288** (0·28)	**0·490** 0·43, 0·45, 0·47 (3)	**1·361** 1·17, 1·19, 1·21 (3)
			Iron in $FeCl_2K_2Cl_2$. **1·637**	**0·014** −0·012, −0·02, −0·03 (2)	**0·228** 0·02, 0·09, 0·14 (3)	**0·302** 0·33, 0·33, 0·33 (3)	**0·504** 0·40, 0·45, 0·48 (3)	**1·375** 1·17, 1·17, 1·17 (3)
		Tin in $SnCl_2KCl$. **1·612**	**0·025** −0·02, 0·03, 0·08 (2)	**0·039** 0·07	**0·248** 0·14	**0·327** 0·33	**0·529** 0·46, 0·56, 0·61 (4)	**1·400** 1·19, 1·24, 1·27 (3)
	Copper in $Cu_2Cl_2K_2Cl_2$. **1·312**	**0·300** 0·19	**0·325** 0·23, 0·27, 0·30 (3)	**0·339** 0·20	**0·548** 0·33	**0·627** 0·54	**0·829** 0·60, 0·66, 0·80 (3)	**1·700** 1·40, 1·44, 1·52 (4)
Silver in $Ag_2Cl_2K_2Cl_2$. **1·172**	**0·140** 0·10	**0·440** 0·36	**0·465** 0·35, 0·40, 0·50 (3)	**0·479** 0·35	**0·688** 0·50	**0·767** 0·59	**0·969** 0·78, 0·80, 0·81 (3)	**1·840** 1·55, 1·57, 1·60 (3)

While the results in Table III are naturally not so regular as those in Table I, most of them are nearer the theoretical values than those of the same combinations in aqueous solutions. In this respect the results for cells in which magnesium or aluminium is concerned are particularly striking. For comparison I take the following numbers (in volts) for metals in aqueous solutions of their chlorides, from Wright and Thompson;* Braun,† has found a similar discrepancy for magnesium in aqueous cells, and attributes it to hydrogen deposited on the magnesium.

	Wright and Thompson. Metals in aqueous solutions of their chlorides.			Metals in fused chlorides. Table III.		
	Calc.	Obs.	Diff.	Calc.	Obs.	Diff.
Magnesium | zinc	1·634	0·702	0·932	1·07	0·87	0·20
Magnesium | cadmium ...	2·000	1·030	0·970	1·15	1·17	−0·03
Aluminium | zinc	1·008	−0·280	1·288	0·20	0·17	0·03
Aluminium | cadmium ...	1·374	0·05	1·324	0·28	0·44	−0·16

A still closer agreement between the observed and theoretical numbers for the metals in Table III is produced by the application of empirical corrections, as in Table IV; and these are of a similar order of magnitude in the two tables, except in the case of zinc.

On testing iron, aluminium, and magnesium for electromotive force due to difference of temperature, iron was found to behave like copper, the hot metal being the negative pole. Aluminium in its double chloride with sodium gave, at first, a strong similar effect; but after some twenty minutes—when a dark deposit, which appeared to contain lead, carbon and iron reduced by the immersed metal, had ceased to separate, and the liquid had become colourless and clear—the effect ceased. It may have been due to impurity in the chloride. With magnesium I could not detect any electromotive force due to difference of temperature. Therefore, considering the uncertainty introduced by these irregularities, and the complication introduced by the use of double salts, cadmium has been allowed to remain as the zero of the scale of corrections, as in Table II.

My experiments were carried out in the laboratory of Queen's College, Belfast, and I am much indebted to Professor Everett for permission to work there and for the use of his electrometer and other apparatus. I am also indebted to Professors Everett and

* 'Phil. Mag.,' ser. 5, vol. 19, pp. 211, 213, 1885.
† 'Wied. Ann.,' vol. 16, p. 578, 1882.

Table IV.

Magnesium. 3·012 − 0·04 = 2·972								
Aluminium. 2·141 + 0·095 = 2·236								0·74 0·75
Zinc. 1 939 + 0·124 = 2·063							0·17 0·17	0·91 0·87
Cadmiur. 1·86						0·20 0·19	0·38 0·44	1·11 1·17
Iron. 1·637 + 0·143 = 1·780					0·08 0·09	0·28 0·33	0·46 0·45	1·19 1·17
Lead. 1·651 + 0·115 = 1·766				0·01 0·02	0·09 0·13	0·30 0·23	0·47 0·45	1·21 1·19
Tin. 1·612 + 0·113 = 1·725			0·04 0·07	0·05 0·03	0·14 0·14	0·34 0·33	0·51 0·56	1·25 1·24
Copper. 1 312 + 0·223 = 1 535		0·19 0·19	0·23 0·20	0·25 0·27	0·33 0·33	0·53 0·54	0·7 0·66	1·44 1·44
Silver 1·172 + 0·236 = 1 408	0 13 0·10	0·32 0·36	0·36 0·35	0·38 0·40	0·45 0·50	0·66 0·59	0·83 0·80	1·56 1·57

G. F. Fitzgerald and to Mr. J. Larmor for several important suggestions.

Since writing the above, my attention has been called to a paper by L. Poincaré,* "Sur les Piles à Electrolytes Fondus et sur les Forces Thermo-électriques à la Surface de contact d'un Métal et d'un Sel Fondu." M. Poincaré has measured the thermo-electric actions in one or two thermo-cells similar to those which I have described, and finds the following thermo-electromotive forces for a difference of 1° between the two metal electrolyte junctions, the sign being that of hotter pole :—

Silver in silver nitrate......	—0·00027	volt.
Zinc in zinc chloride.......	+0·00013	„
Tin in stannous chloride....	0·000028	„

The sign for tin does not appear to be given; that for zinc is the reverse of my result. M. Poincaré has also measured the electromotive force of one voltaic cell with fused electrolytes,

Zinc | $ZnCl_2$ | $SnCl_2$ | Tin,

and finds it to be 0·335 volt. My result, in Table I, when reduced to volts, is 0·350. He finds for the value just after solidification of the electrolyte, 0·37 volt, and considers that, in order to obtain the true heat equivalent of a cell with fused electrolytes, the latent heat of fusion of the salts used should be deducted from Thomsen's heats of combination for the solid salts. There would appear to be, on theoretical grounds, some doubt on this point. It could be experimentally tested more definitely if data for these heats of fusion were available.

Synopsis.

In instituting a comparison of the observed electromotive forces of cells with their theoretical values, as given by Thomson's law, the simpler the construction of the cell the more easy is it to ascertain the nature of the chemical action going on in it, upon which the calculation rests.

In cells with aqueous electrolytes, the solvent introduces several irreducible complications, arising from possible action of the solvent itself as an electrolyte, from the formation of insoluble films (either inactive and protective as oxides, or active, as hydrogen), or from the uncertainty of calculations involving heats of solution of the products of voltaic reaction.

When the liquidity of the electrolyte is produced by fusion instead

* 'Comptes Rendus,' vol. 110, p. 339, 1890.

of solution, these complications are, to a large extent, avoided; and, as a matter of experiment, two fluid cells made up of metals immersed each in its own fused chloride, give results nearer the theoretical values than those obtained with aqueous solutions. This is specially noticeable in the case of metals with high heats of oxidation as in the case of magnesium and aluminium.

The electromotive forces of cells containing pairs of the metals, tin, lead, and zinc, come out nearest to the theoretical values. The other metals which were tried did not give so close an agreement, but can be brought into agreement by applying constant corrections, one for each metal; and reasons are given for attributing these corrections to the high temperatures of the cells as compared with the temperatures for which the recognised heats of combination are true.

Four of the cells were tested by passing currents through them in both directions alternately, and noting the electromotive force after the passage of each current. Polarisation was observed to a small extent in one case—zinc-silver; in the others it was practically absent.

IV. "The Physiological Action of the Nitrites of the Paraffin Series considered in connexion with their Chemical Constitution. Part II. Action of the Nitrites on Muscular Tissue and Discussion of Results." By J. THEODORE CASH, M.D., F.R.S., Professor of Materia Medica in the University of Aberdeen, and WYNDHAM R. DUNSTAN, M.A., Professor of Chemistry to the Pharmaceutical Society of Great Britain. Received June 15, 1892.

(Abstract.)

Continuing the examination of the physiological action of various pure organic nitrites of the paraffin series (Part I; 'Roy. Soc. Proc.,' 1891), the authors have studied their effect on striated muscular tissue. When the vapours of these nitrites come into contact with the muscle a paralysant effect is observed. All the experiments were made with the triceps and gastrocnemius of *Rana temporaria*. The muscle was contained in a specially constructed air-tight chamber, which, whilst it admitted of connexion being made between the muscle and the recording apparatus, rendered it possible to bring into contact with the muscle the vapour of a known quantity of the nitrite without any loss taking place. A very extensive series of experiments was necessary, as it is unsafe to contrast the gastrocnemius of one frog with that of another, so that the action of every

member of the series of nitrites had to be contrasted with each of the other members on the companion gastrocnemii of the same animal, and each experiment was repeated three times, and often five or six times. The amounts of the nitrites employed varied between $\frac{1}{80}$ and $\frac{1}{400}$ c.c.

Several series of concordant results have thus been obtained which lead to two different orders of activity, viz., (1) with reference to the extent to which equal quantities of nitrites shorten the resting muscle, and (2) with reference to the rapidity with which the shortening is produced. The order of activity of the nitrites as regards the extent of the shortening they induce is as follows:—(i) Iso-butyl, (ii) tertiary amyl, (iii) secondary butyl, (iv) secondary propyl, (v) propyl, (vi) tertiary butyl, (vii) butyl (viii) α-amyl, (ix) β-amyl, (x) ethyl, (xi) methyl. The order representing the speed with which shortening occurs is (i) methyl, (ii) ethyl, (iii) secondary propyl, (iv) tertiary amyl, (v) primary propyl, (vi) tertiary butyl, (vii) secondary butyl, (viii) α-amyl, (ix) β-amyl, (x) primary butyl, (xi) iso-butyl.

The effect of these nitrites in interfering with the active contraction of a stimulated muscle has also been studied, and it has been ascertained that very minute doses, insufficient to cause passive contraction, interfere in a marked degree with the active contraction and cause the muscle to fail in responding to stimulation, whilst the companion muscle, contained in a closed chamber free from nitrite vapour, still responded to stimulation.

The remainder of the paper is devoted to a discussion of the connexion between the various phases of physiological action and the chemical constitution of the nitrites which give rise to them. This discussion cannot be adequately abstracted. The principal conclusions which have been arrived at are briefly as follows:—The physiological action of these nitrites is not solely, and in some cases not even mainly, dependent on the amount of nitroxyl (NO_2) they contain.

In respect of all phases of physiological activity, the secondary and tertiary nitrites are more powerful than the corresponding primary compounds. This is to be chiefly attributed not to the direct physiological action of the secondary and tertiary groups, but to the great facility with which these compounds suffer decomposition mainly into the alcohol and nitrous acid. In respect of the acceleration of the pulse, the power of the nitrites is directly as their molecular weight, and inversely as the quantity of nitroxyl they contain. They, therefore, fall into an order of physiological activity which is identical with that in which they stand in the homologous series. This same relationship, increase of physiological activity corresponding with rise in molecular weight, may also be traced, though less uniformly,

in their power of reducing blood-pressure, and of inducing muscular contraction.

This order appears to be the result not so much of the direct physiological influence of the substituted methyl groups as of the increased chemical instability which their presence confers on the higher members of the series. In respect of the duration of subnormal pressure, as well as of the rapidity with which muscular contraction ensues, the activity of the nitrites is expressed by an order which is for the most part the reverse of that representing their power in accelerating the pulse, reducing blood-pressure, and contracting muscular fibre, this order being in general contrary to that of the homologous series. In these respects the more volatile nitrites of low molecular weight which contain relatively more nitroxyl are the most active. It appears probable that these simpler nitrites more readily attach themselves to certain constituents of blood and muscle, and thus act more quickly than the higher compounds, whilst their greater stability causes their effects, *i.e.*, reduction of blood-pressure, &c., to endure for a greater length of time than that of the higher and more easily decomposed bodies.

A large proportion of an organic nitrite is changed into nitrate in its passage through the organism, and is excreted as an alkali nitrate in the urine.

The results which have been gained by this research have an important bearing on the therapeutic employment of the nitrites. It is proposed elsewhere to consider what the outcome of this investigation is for practical medicine.

V. "On the Estimation of Uric Acid in Urine: a New Process by means of Saturation with Ammonium Chloride." By F. GOWLAND HOPKINS, B.Sc., Gull Research Student at Guy's Hospital. Communicated by Dr. PYE SMITH, F.R.S. Received May 30, 1892.

The process to be described depends upon the following facts:—

1. Ammonium urate is wholly insoluble in saturated solutions of ammonium chloride.

2. If solutions, such as urine, which contain the mixed urates of different bases be saturated with ammonium chloride, the large mass-influence of the latter ensures the rapid and complete conversion of all the uric acid into biurate of ammonium, which, in accordance with (1), is, *pari passu*, thrown out of solution. In the case of urine, saturation with ammonium chloride is followed by a complete precipitation of the uric acid present in the course of two hours at most.

It has long been known that ammonia, and salts of ammonia, will, after long standing, so completely precipitate the uric acid from urine that subsequent treatment with acids produces no further precipitate (Wetzlar, 'Beiträge zur Kenntniss des menschlichen Harns,' Frankfurt a.M. 1821, p. 19). Fokker, moreover, observed that when a solution of any urate is made alkaline, and then mixed with a solution of ammonium chloride, the uric acid slowly separates out as the biurate of ammonia. Upon this observation the well-known Fokker-Salkowski process is based.

But the crucial fact that *saturation* with the ammonium chloride renders the separation rapid and absolutely complete does not seem to have been previously observed. The importance of this fact in any attempt to estimate uric acid by means of its separation as ammonium urate will be seen when it is remembered that, hitherto, in all such processes a large correction has to be made to allow for incomplete separation, even after long standing. In the Fokker-Salkowski process, for instance, the factor which has to be added to the result amounts to, at least, 25 per cent. of the total variable dealt with, a necessity which would seem to render the process of little scientific value (*vide* Fokker, Pflüger's 'Archiv,' vol. 10, p. 157, 1875; Salkowski, Virchow's 'Archiv,' vol. 68, p. 401, 1876). In the method about to be described no such correction is necessary.

The following experiments with pure urates will show the complete separation of ammonium urate which occurs after saturation with the chloride.

In each case pure uric acid, prepared by means of the sulphate, was used. The ammonium salt precipitated was decomposed by means of HCl, and the uric acid subsequently washed and weighed, due allowance being made for washings:—

	Uric acid recovered.
(1.) One decigramme of uric acid dissolved in ammonia, solution made up to 250 c.c., saturated with ammonium chloride, and filtered after 30 minutes	0·0994
(2.) Fifty milligrammes dissolved as above and made up to 150 c.c. Filtered after 15 minutes	0·0502
(3.) One decigramme dissolved in Na_2Co_3, solution made up to 100 c.c. Filtered after 3 hours	0·0986
(4.) One decigramme dissolved as in (3). Filtered after 4 hours	0·0990

The presence of excess of bases, other than ammonia, delays the

precipitation, but, as stated above, two hours is amply sufficient in the case of urine. (*Vide* note (*b*), *infra*).

The process of estimation in urine may be carried out as follows:—

To 100 c.c. of the urine, finely-powdered pure ammonium chloride is added in excess, about 30 grammes being necessary. The solution is allowed to stand for two hours, with occasional stirring, and the precipitate then filtered off through a thin filter paper.

The precipitate is then washed with a saturated solution of ammonium chloride. If a few crystals of the solid salt used for precipitation are still undissolved at the time of filtration, it is of no disadvantage; the light flocculent urate is easily washed first on to the filter, the crystals being then dissolved in a minimum of distilled water and used for washings.

After it has been washed twice or thrice with the saturated chloride solution, the urate is washed off the paper into a small beaker by means of a jet of hot distilled water.

The ammonium urate is then decomposed by boiling with a slight excess of HCl, the solution concentrated, if necessary, and the uric acid allowed to separate out. It may be finally determined by any accustomed method. (*Vide*, however, note (*c*), *infra*).

Notes.

(*a*.) The ammonium chloride used must be pure. Since 30 grammes are used to precipitate 100 c.c. of urine, and only some 30—40 milligrammes of uric acid finally weighed, it is obviously fatal to the accuracy of the process if even a very small percentage of insoluble matter is present in the salt. In some samples, sold as pure, I have found appreciable quantities of sand, and, in others, an amount of fibre from the filter used in the preparation of the salt equal to 2—3 milligrammes in the 30 grammes. Other mineral salts of ammonia will precipitate the urate on saturation, but none are so satisfactory as the chloride.

(*b*.) The presence of free ammonia greatly accelerates the separation of ammonium urate after saturation with ammonium chloride.

The addition of AmHO to urine obviously involves the separation of phosphates. But, if the samples be first saturated with the chloride, and the hydrate subsequently added, the separation of urinary phosphates is greatly modified. No gelatinous precipitate comes down, but only a small quantity of triple phosphate crystals, such as neither adds appreciably to the bulk of the urate precipitate nor increases the difficulty of filtration (this is rendered evident if the precipitate produced by AmCl in an acid urine be first filtered off and AmHO added to the filtrate).

If, therefore, time is of special importance, AmHO may be safely

added after saturation with AmCl. The urate, *plus* phosphate, may now be filtered off within ten minutes, and the uric acid liberated in the usual way.

In describing the process above, standing for two hours is recommended where AmCl is added alone. In a very large percentage of urines, however, if not in all, one hour will be found enough.

(*c.*) *The final determination of the separated uric acid* may, of course, be carried out by means of weighing on a tared filter paper, or on a Ludwig's glass-wool filter.

Personally, however, I greatly prefer to filter the crystals through a small filter of the kind sold by Schleicher and Schüll, and called by them "Gehärtete" filter paper. From these the uric acid may be very easily washed, without loss, into a weighed glass basin. The washings are then evaporated to dryness on the water-bath, the residue dried at 110° C., and weighed.

A Volumetric Method.—Recent experience has convinced me of the utility of permanganate solutions (*vide* Sutton's 'Volumetric Analysis,' 4th ed., p. 89) for the accurate titration of urates; so much so that I believe uric acid, once separated, by any method, from admixture with other organic substances, may be better titrated than weighed.

At temperatures ranging round 60° C. a perfectly determinate reaction occurs, involving the instantaneous decolorisation of the permanganate; and, though oxidation continues on standing greatly beyond the limits of this reaction, a perfectly definite end-point is obtainable, giving results which will be found extremely accurate.

The uric acid separated, as above, from 100 c.c. of urine is, after washing, dissolved by warming with a minimum of Na_2CO_3. The solution is then cooled to about 15° C., made up to 100 c.c., transferred to a flask, rapidly mixed with 20 c.c. of pure strong sulphuric acid, and then *immediately* titrated with one-twentieth normal potassium permanganate solution. The addition of 20 per cent. of sulphuric acid produces just such a temperature as is required for the reaction. The end-point is marked by the first appearance of a permanent pink flush. On standing, the colour continues to disappear, but this slow decolorisation is in marked contrast with the instantaneous nature of the first reaction. Careful experiments with pure urates have convinced me that uric acid may be thus estimated to within less than half a milligramme.

One c.c. of twentieth normal permanganate of potassium, made by dissolving 1·578 grammes of the salt in 1 litre of distilled water, is equal to 0·00375 gramme uric acid.

In the 'Guy's Hospital Reports' for 1891 I have described a method in which the ammonium urate precipitate, obtained by saturation with AmCl, is directly titrated with permanganate. As this precipi-

tate always contains a trace of pigment, the results are somewhat high, but the method will be found sufficiently accurate for clinical purposes.

Evidence as to the Accuracy of the Ammonium Chloride Separation when applied to Urine.

(1.) After urine has been saturated with the chloride, allowed to stand, as described, and filtered, uric acid cannot be detected by any process in the filtrate.

(2.) The uric acid as weighed gave the following figures on combustion. Mixed residues from actual determinations were used:—

	Found.		
	(*a.*)	(*b.*)	Theory.
C........	35·460	35·990	35·71
H........	2·561	2·590	2·381
N........	32·400	31·980	33·3

The above nitrogen figures show some variation from theory, but it should be remembered that elementary analysis is a very rigorous test when applied to such unpurified products. I have never obtained figures so near to the truth from the product weighed in the Ludwig process.

(3.) Comparison with the standard Ludwig-Salkowski process:—

Series 1.

	Milligrammes per 100 c.c.	
	Ludwig.	AmCl.
Sample 1	23·0	23·0
„ 2	46·0	49·0
„ 3	55·0	57·5
„ 4	59·5	61·5
„ 5	64·0	68·0
„ 6	80·5	82·5

Series 2.

„ 7	70·5	71·0
„ 8	54·5	54·5
„ 9	40·5	39·5
„ 10	40·0	40·5

The experiments in Series 2 will be seen to afford nearer agreement than the earlier series, carried out before the same amount of experience had been obtained. The better agreement depends partly

upon the use of a purer ammonium chloride, but chiefly upon a precaution in Ludwig's method to which reference is made below.

Notes on other Processes.

Having regard to the discussion at present going on with respect to the whole question of uric acid estimation, it may not be out of place to chronicle such experience of other processes as has been gained in the course of working out the method just described.

In comparing the ammonium chloride figures with those of Ludwig's process, the experiments marked Series 1 above indicated an excess by the former method equal to some 3 milligrammes from the 100 c.c. of urine. As stated above, a part of this excess was due to impurities in the ammonium chloride, but it was mainly due, I believe, to a destruction of uric acid which occurs in Ludwig's process when the silver urate is treated with the alkaline sulph-hydrate. It is, of course, well known that uric acid is decomposed on heating with caustic alkalies, but it is hardly recognised with what great ease the decomposition takes place. I find that, on boiling 50 milligrammes of pure uric acid briskly for five minutes with 10 c.c. of Ludwig's sulphide solution (*vide* Neubauer and Vogel, 'Analyse des Harns,' p. 543), *i.e.*, the amount used to decompose the silver salt from 100 c.c. of urine, there is always an appreciable loss, sometimes amounting to 3 milligrammes. It is therefore obvious that even five minutes' boiling would be fatal to the accuracy of an estimation. In Series 2 above, the silver compound was thoroughly shaken up with the sulphide in the cold, and only just heated to boiling before filtration.

Mr. E. Groves, B.Sc., has recently described ('Journal of Physiology,' December, 1891) a modification of Ludwig's method which I have several times employed. Mr. Groves uses potassium iodide instead of the sulphide to decompose the silver urate. In my hands the method has always given results which are decidedly too low, owing doubtless to the action of the liberated iodine upon the uric acid in the acidified filtrate.

I have performed a large number of experiments with Haycraft's method, and have found that results obtained by its means are invariably too high, while the proportionate error undoubtedly appears to be variable. This appears to be the common experience. My own experiments, however, lend no support to the extraordinarily wide variations from the truth which some observers, especially in England, have ascribed to it. I should put the limit of error at from +10 to +15 per cent.

In conclusion, it may be claimed for the separation by means of saturation with chloride of ammonium that it yields results which

are at least as accurate as those of Ludwig's method, while it is far easier to carry out, and occupies much less time.

VI. "On the Potential Difference required to produce a Spark between two Parallel Plates in Air at Different Pressures." By J. B. PEACE, M.A., Fellow of Emmanuel College, Cambridge. Communicated by Professor J. J. THOMSON, F.R.S. Received May 24, 1892.

Introductory.

The following is an account of an investigation into the relations between potential difference, spark-length, and pressure when discharge takes place between two parallel plates in air. The investigation was undertaken at the suggestion of Professor J. J. Thomson, and has been carried out in the Cavendish Laboratory in Cambridge.

Preliminary experiments were made in July and August, 1890, in which a small Wimshurst machine was used, and the potential difference was measured by one of Lord Kelvin's vertical electrostatic voltmeters. For sparks of some length fairly consistent results were obtained, but in the case of short sparks accurate measurement of the potential difference was difficult. Use was then made of a large number of small secondary cells, and, at the same time, more accurate measurements of the spark-length were made. At first about 500 cells were used, so that steady potential differences, ranging up to about 1,000 volts, could be maintained, and sparking differences up to about 0·1 mm. were examined. The results of these experiments showed very fair agreement with those of other observers; they were sufficiently consistent among themselves to show that the cells could be relied on. More cells were therefore set up, and all were carefully tested as to equality, rate of fall of charge, &c.

Apparatus was then constructed by which discharge at different pressures could be examined. Description of this apparatus, which is sketched in fig. 1, and an account of the method and results of observations made with it in July and August, 1891, are given in Part I below. At first some difficulty was found in making the bell-jar and its connexions sufficiently air-tight, but when this was overcome an effect similar to that observed by De la Rué and Müller in discharge in exhausted tubes became apparent; that is, it was found that, as the pressure diminished, the potential difference required to produce a spark across a given distance fell to a minimum value, and then began to increase rapidly. To examine this effect more fully, the observations were confined to pressures ranging from 20 to

300 mm. of mercury, the water-pump which was used, and the stuffing-box through which the sparking distance was adjusted, not admitting of observations at lower pressures. The results of these observations are tabulated below, and appear in the curves in figs. 2, 3, and 4.

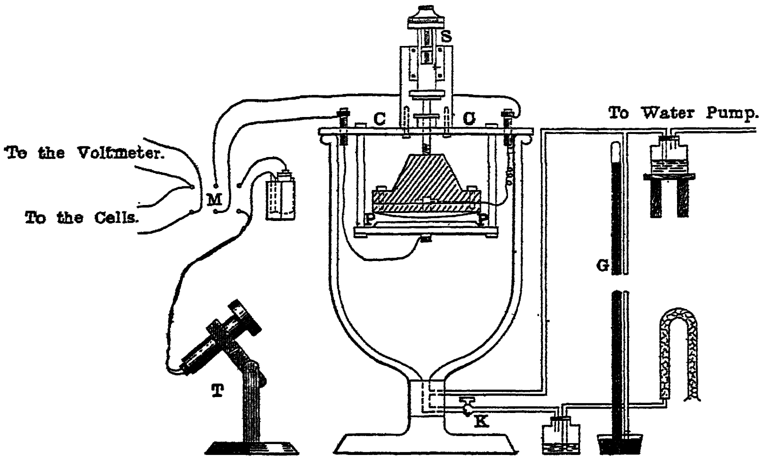
When these results were examined and compared, it was found that the curves connecting potential difference and pressure not only exhibited the minima above mentioned, but also crossed each other, showing that at low pressures the shorter spark required the greater potential difference. As this seemed a point of some importance, it was decided to make a more direct investigation, and with this view apparatus was constructed and observations made in April, 1892. This apparatus is described, and the results obtained are recorded in Part II below. The observations were confined to pressures ranging from 2 or 3 up to 50 mm. of mercury, and the results confirmed and extended those formerly obtained.

Part I.

A sketch of the apparatus used is given in fig. 1. An inverted bell-jar, communicating with a water-pump, had its mouth ground and fitted with a brass cover, C. This cover carried the attachments of the discharge plates P, P, and the micrometer screw S. The screw had a pitch of $\frac{1}{50}$ inch, and its head was graduated in hundredths of a revolution. A short steel rod, fixed to the sliding frame of the screw and working through a stuffing-box in the centre of the cover C, carried an insulating block, to which the upper discharge plate could be readily attached. The lower plate was fixed to an ebonite disc which was rigidly suspended from the cover C. The plates were of brass, highly polished, and were about $2\frac{5}{8}$ inches in diameter. The lower plate was plane, the upper was slightly convex, having a radius of curvature of about 9 inches. Short flexible wires passed from the plates to insulated terminals passing through the cover. From these terminals leads passed to mercury cups, M, by which connexion could be made, on the one hand with the leads of the telephone T, and, on the other hand, with leads passing to the secondary cells and to the voltmeter.

The cells were arranged in groups of eighteen in series; for charging, the groups were arranged in parallel; for spark discharge they were arranged in series, while the leads passing to M could be connected to the terminals of any required number of cells. In the discharge circuit there were also a simple make-and-break key and a very high resistance, the latter to prevent the setting up of an arc discharge between the plates. This resistance was usually a black-lead line on a strip of ebonite.

FIG. 1.



The path from the bell-jar to the pump passed through sulphuric acid, and was also connected to a mercury gauge, G. Air could be admitted to the jar through the stop-cock K, after passing through cotton wool and sulphuric acid.

The following was the usual course of a series of observations:— While the cells were charging, which generally occupied about one half hour, the plates were polished and fixed in position, the cover fitted to the mouth of the bell-jar, and the pump put in action. The screw reading for contact of the plates was taken, the upper plate being moved downwards till contact took place and completed the telephone circuit. The plates were set at any required distance apart, the upper plate being, in all cases, moved downwards to its final position.

When the cells were charged they were allowed a few minutes to reach a steady state and were then joined up in series, and various groups of 500 tested by the electrostatic voltmeter; in this, the smallest weight being used, one scale division read 50 volts, so that direct reading gave ten times the voltage of a single cell. This measurement was repeated at intervals during each experiment, and only those observations were preserved in which the cells either remained practically constant, or fell in potential difference so slowly and uniformly that accurate allowance could be made.

The plates being now at a known distance and the air at a known pressure, regulated by the pump on the one hand and the stop-cock K on the other, the number of cells connected to the plates, always at first well under what was required to produce discharge, was gradually increased until the spark passed, and the potential difference

was taken as being proportional to the number of cells. The observations were made for a series of pressures ranging from 20 to 300 mm. of mercury.

When the apparatus was first used, a fixed potential difference was given to the plates, and the pressure gradually lowered until a spark passed. When, however, it was found that, in certain circumstances, a diminution of pressure required an increase of potential difference, this method was abandoned, and that described above was adopted.

Within the specified range of pressure a considerable number of observations were made in July and August, 1891, for spark-lengths corresponding to 2, 5, 10, 20, 40, 100, 200, and 400 divisions of the screw head. The mean results of these observations are given in the following tables. Each table represents the mean of at least three series of observations, taken at considerable intervals of time, and generally showing very close agreement with each other.

From the observed potential difference the electrostatic force has been deduced, that is, the potential difference (in electrostatic units) per centimetre of spark-length. The electrostatic force is given in the third column of the following tables, the first column containing the air pressure in millimetres of mercury, and the second the potential difference in volts, while d is the spark-length, as measured above.

The tables are represented by the curves of figs. 2 and 3, fig. 2 showing the connexion between potential difference and pressure for various constant spark-lengths, and fig. 3 showing the connexion between electrostatic force and pressure for the same spark-lengths. From these are deduced the curves in fig. 4, showing the relation between potential difference and spark-length for various constant pressures.

Curves A, fig. 2, and A, fig. 3. $d = 0{\cdot}0004$ in.			Curves B, fig. 2, and B, fig. 3. $d = 0{\cdot}001$ in.		
Pressure, mm. of mercury.	Potential difference, volts.	Electrostatic force, C.G.S. units.	Pressure, mm. of mercury.	Potential difference, volts.	Electrostatic force, C.G.S. units.
20	433	1420	20	430	564
30	398	1310	30	396	528
40	380	1245	40	377	495
50	370	1215	50	361	474
60	357	1170	60	354	465
70	353	1160	70	347	456
80	349	1145	80	343	450
90	346	1135	90	339	445
100	343	1125	100	336	440
120	337	1105	120	334	438
140	332	1090	140	331	434
160	330	1085	160	332	435
180	329	1080	180	334	438
200	328	1075	200	338	444
240	326	1070	250	352	463
280	327	1072	300	376	495
300	328	1075			

Curves C, fig. 2, and C, fig. 3. $d = 0{\cdot}002$ in.			Curves D, fig. 2, and D, fig. 3. $d = 0{\cdot}004$ in.		
Pressure, mm. of mercury.	Potential difference, volts.	Electrostatic force, C.G.S. units.	Pressure, mm. of mercury.	Potential difference, volts.	Electrostatic force, C.G.S. units.
20	426	278	20	421	138
30	393	265	30	383	126
40	373	244	40	365	120
50	360	236	50	355	116
60	352	231	60	355	116
70	344	225	70	360	118
80	340	223	80	369	121
90	336	220	90	380	125
100	334	218	100	390	128
120	334	218	120	416	136
140	340	223	140	442	145
160	348	228	160	470	154
180	358	235	180	491	161
200	374	245	200	515	169
250	414	271	250	573	192
300	456	300	300	626	205

Curves E, fig. 2, and E, fig. 3. $d = 0{\cdot}008$ in.			Curves F, fig. 2, and F, fig. 3. $d = 0{\cdot}02$ in.		
Pressure, mm. of mercury.	Potential difference, volts.	Electrostatic force, C.G.S. units.	Pressure, mm. of mercury.	Potential difference, volts.	Electrostatic force, C.G.S. units.
20	410	67	20	484	31·5
30	373	61	30	510	33·5
40	375	61	40	550	36·0
50	385	63	50	580	38·0
60	402	66	60	617	40·5
70	420	69	70	660	43·0
80	435	71	80	699	46·0
90	451	74	100	778	51·0
100	468	77	120	852	56·0
120	507	83	140	933	61·0
140	541	89	160	1000	65·5
160	575	94	200	1138	74·0
180	607	99	220	1208	79·5
200	645	103	240	1272	83·5
250	729	119	260	1340	88·0
300	802	132	280	1416	92·5
			300	1490	97·5

Curve G, fig. 2. $d = 0{\cdot}04$ in.			Curve H, fig. 2. $d = 0{\cdot}08$ in.		
Pressure, mm. of mercury.	Potential difference, volts.	Electrostatic force, C.G.S. units.	Pressure, mm. of mercury.	Potential difference, volts.	Electrostatic force, C.G.S. units.
20	528	17·3	16	550	9·0
30	589	19·3	20	635	10·4
40	662	21·8	30	800	13·1
50	731	24·0	40	950	15·6
60	824	27·0	50	1100	18·1
70	906	29·5	60	1255	20·5
80	970	31·8	65	1326	21·7
90	1046	34·2	75	1464	24·0
100	1116	37·2			
120	1260	41·4			
130	1326	43·5			

FIG. 2.

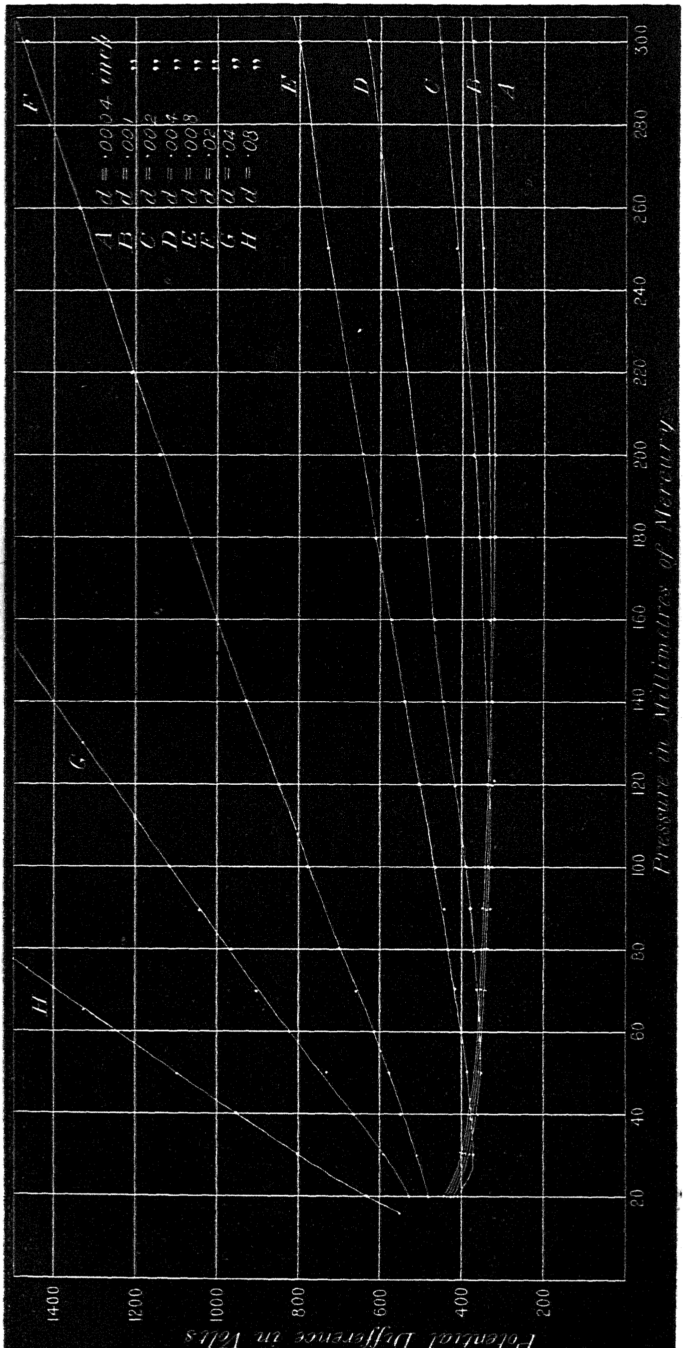
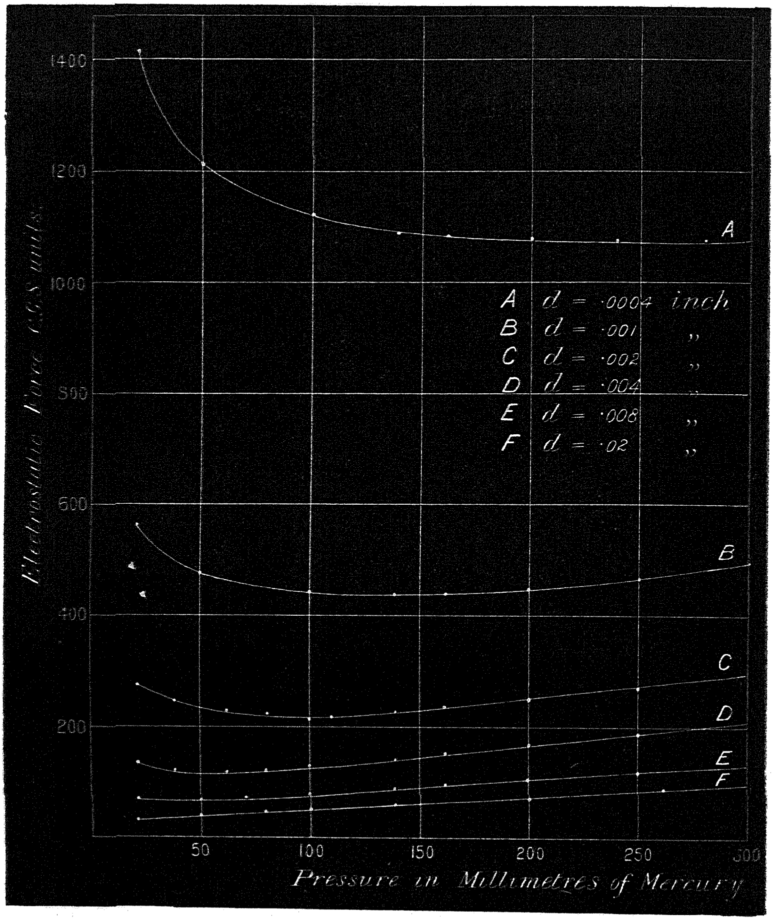

A d = ·0004 inch
B d = ·001 „
C d = ·002 „
D d = ·004 „
E d = ·008 „
F d = ·02 „
G d = ·04 „
H d = ·08 „
A
B
C
D
E
F
G
H
20
40
60
80
100
120
140
160
180
200
220
240
260
280
300
Pressure in Millimetres of Mercury
200
400
600
800
1000
1200
1400
Potential Difference in Volts

FIG. 3.

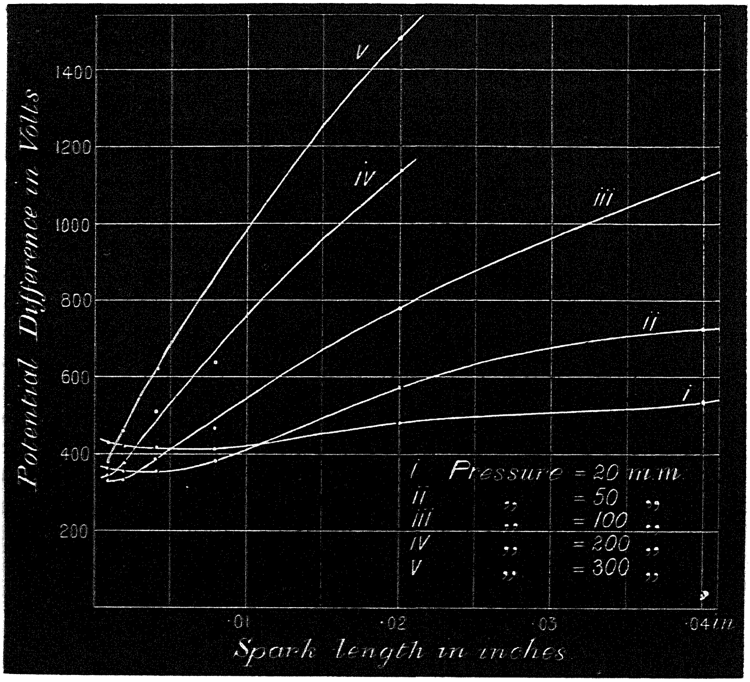


These results, and the curves representing them, present two features, already mentioned, which call for remark. In the first place, the potential difference curves in fig. 2 exhibit minima at pressures which are relatively high. The following table (p. 107) gives the minima shown by the curves, as drawn in fig. 2.

In every series of observations these minima were well marked, and for different series the pressures corresponding to the minimum potential difference for a given spark-length were always in close agreement. These minima are considered again below, in connexion with the results of Part II.

In the second place, the potential difference curves in fig. 2 meet

Fig. 4.



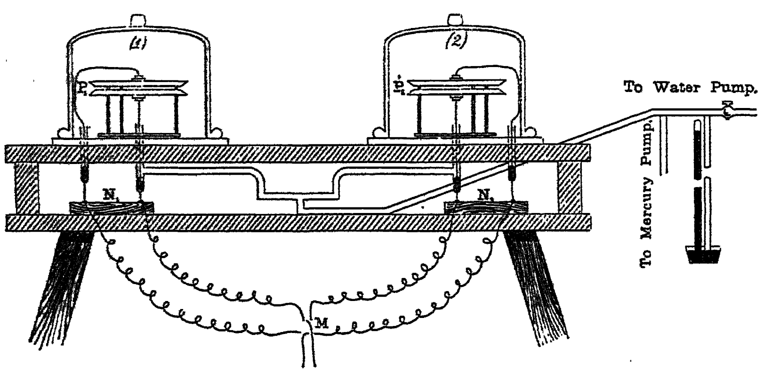
Spark-length, inches.	Minimum potential difference, volts.	Pressure, mm. of mercury.
0·0004	326	240
0·0010	330	150
0·0020	333	110
0·0040	354	55
0·0080	370	33

and pass above each other, or, which is the same thing, the curves in fig. 4, showing the relation between potential difference and spark-length for given pressures, also exhibit minima. This effect, though appearing consistently, was not very distinctly marked, but there were indications that it would be more apparent at lower pressures, and, therefore, it was thought advisable to submit it to more direct investigation. For this purpose new apparatus was fitted up, as described below.

PART II.

The apparatus used is sketched in fig. 5. Two pairs of plates at different distances apart, but with same air pressure, were arranged in parallel, so that an alternative path was offered to the discharge. The two pairs of plates were enclosed in two small receivers. The lower plate in each rested on an insulating stand; the upper plate was supported and separated from the lower by small thin discs of ebonite, mica, or glass. The four plates used were plane, and the thickness of the separating discs, as measured by the screw calipers, is taken as the length of the spark. The receivers rested on well-ground brass base plates, into each of which were sealed two narrow glass tubes, containing mercury, and having sealed platinum wires passing to the outside; flexible wires passed from the discharge-plates and dipped into the mercury in the tubes. One of these tubes served also, in each case, to give connexion with the pumps. Two pumps were used, the water pump for rapid exhaustion to about 30 or 40 mm. of mercury, the mercury pump for lower pressures. Pressures of from 2 to 3 mm. of mercury were easily reached, and could be maintained for any length of time.

FIG. 5.



The platinum wires, in connexion with the plates, dipped into mercury cups in the paraffin blocks N_1 and N_2; from these leads passed to the block M, where either one pair of plates, or both, could be connected to the leads passing to the cells. The cells were used in the same manner as before; the potential difference, however, instead of being reckoned as proportional to the number of cells applied, was measured directly by a cylinder quadrant electrometer. This was used in order to avoid a fresh examination into the uniformity of the cells.

The apparatus was set up, and the observations tabulated below were made in April, 1892. These observations were confined to pressures lower than 50 mm. In each case the two pairs of plates had air-gaps of very different length, so that each series of observations amounted to a determination of two separate potential difference and spark-length curves. The sparks were separately examined, until pressures were reached at which the potential differences necessary for discharge became nearly equal, and then the two pairs of plates were simultaneously connected to the cells. The crossing of the potential difference curves was thus directly verified, a slight lowering of the pressure transferring the discharge from the shorter to the longer air-gap.

The plates used in this experiment were, all four, plane and polished. Two were of brass, one of copper, and one of zinc. When the different plates were interchanged between the long spark-gap and the short, no difference was observed in the potential difference required for discharge at given difference and air-pressure. That there might be no doubt as to whether the discharge was passing actually between the plates, or was taking a longer path from edge to edge, the edges of the plates were only very slightly rounded off. At low pressures there was considerable tendency to a brush discharge from the edges, but this was considerably reduced by an extremely thin coating of shellac varnish.

The results given below are those of three single series of observations made on April 21, 22, and 23, respectively. All of these were repeated on the same or other days, and in all cases there was almost exact agreement. For convenience of reference these series are called A, B, and C, the suffix 1 referring to the left-hand, and the suffix 2 to the right-hand, spark-gap. As in the former case, the tables give corresponding pressure, potential difference in volts (V), and electrostatic force in C.G.S. units (F). Curves, similar to those given in fig. 2 and 3, representing potential difference and pressure, and electrostatic force and pressure, are also given in figs. 6 and 7.

A.

Pressure, mm. of mercury.	Curves A_1, fig. 6, and A_1, fig. 7. $d = 0·03$ in.		Curves A_2, fig. 6, and A_2, fig. 7. $d = 0·082$ in.	
	V_1, volts.	F_1, C.G.S. units.	V_2, volts.	F_2, C.G.S. units.
50·0	611	26·7	983	15·7
28·0	494	21·6	712	11·4
15·0	438	19·1	539	8·6
10·0	428	18·7	488	7·8
5·5	462	20·2	480	7·7
2·5	731	32·0	649	10·4

B.

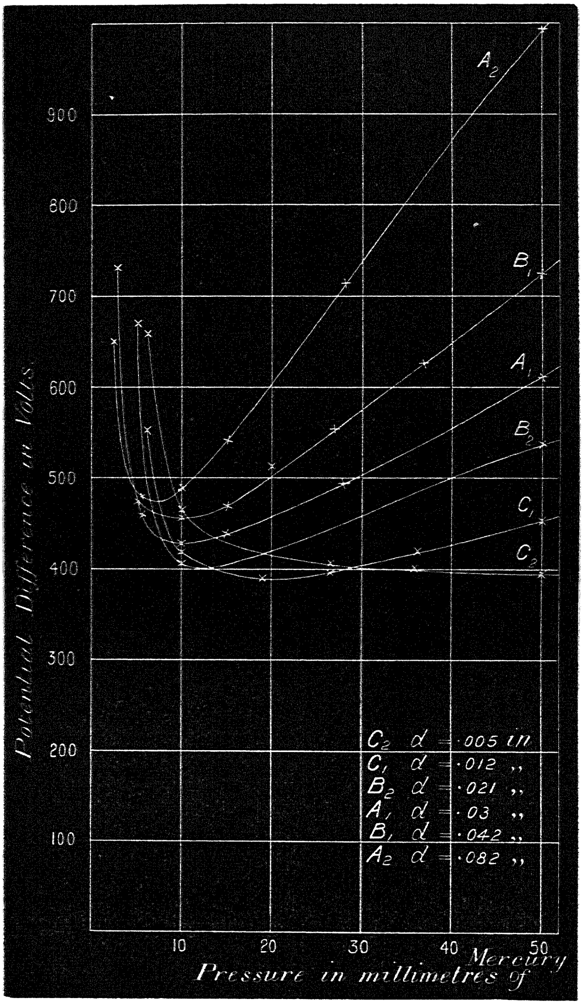
Pressure, mm. of mercury.	Curves B_1, fig. 6, and B_1, fig. 7. $d = 0·042$ in.		Curves B_2, fig. 6, and B_2, fig. 7. $d = 0·021$ in.	
	V_1, volts.	F_1, C.G.S. units.	V_2, volts.	F_2, C.G.S. units.
50	726	22·7	535	33·4
37	626	19·6	488	30·5
27	554	17·3	446	27·8
20	513	16·1	419	26·1
15	469	14·7	405	25·1
10	458	14·3	407	25·4
5	478	14·9	670	41·9

C.

Pressure, mm. of mercury.	Curves C_1, fig. 6, and C_1, fig. 7. C_1 $d = 0·012$ in.		Curves C_2, fig. 6, and C_2, fig. 7. C_2 $d = 0·005$ in.	
	V_1, volts.	F_1, C.G.S. units.	V_2, volts.	F_2, C.G.S. units.
50·0	454	49·6	397	104
36·0	422	46·2	402	105
26·0	397	43·5	408	107
19·5	389	42·6	414	109
10·0	420	46·0	467	122
6·0	552	60·6	660	173

It ought to be mentioned that the values here given for the spark-length d can only be regarded as approximations, as the pressure applied in measuring the thickness of the separating disks by the

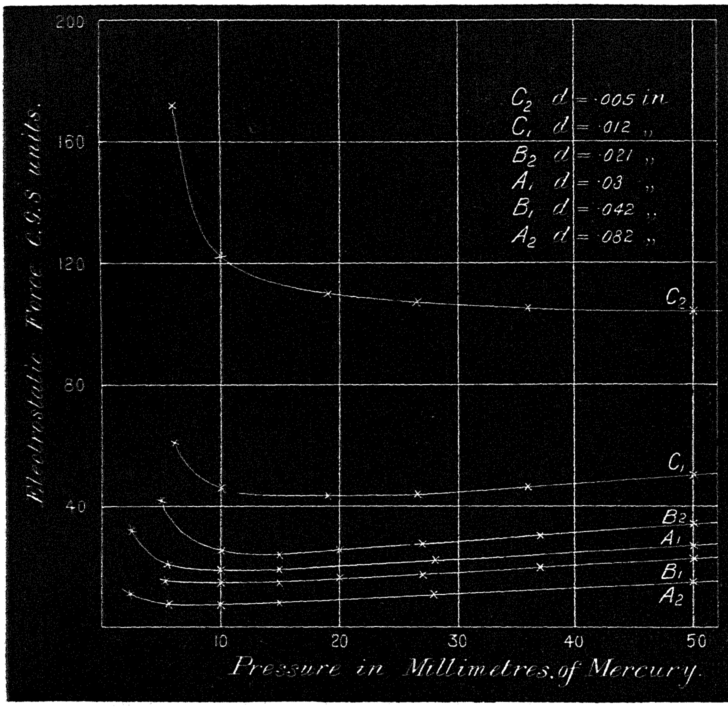
FIG. 6.



screw caliper might be very different from that due to the weight of the supported plate, especially in those cases in which several thicknesses of mica or ebonite were used. The essential point of this part of the investigation, however, was not the accurate determination of spark-length, but the direct comparison of discharge across two spark-gaps known to be of very different length.

The following table, similar to that given on p. 107, gives corresponding values for minimum potential difference, spark-length, and pressure, as deduced from the curves of fig. 6 :—

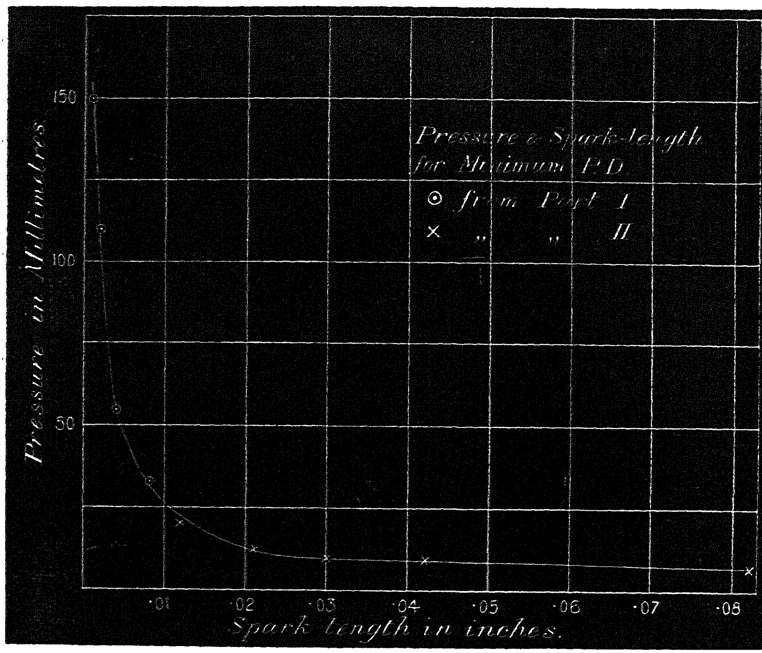
FIG. 7.



Spark-length, inches.	Minimum potential difference, volts.	Pressure, mm. of mercury.
0·012	390	19
0·021	400	12
0·030	428	10
0·042	458	9
0·082	475	7

The curve given in fig. 8 represents corresponding values of air pressure and spark-length for minimum potential difference. The points marked on this curve by a dot are taken from the results of Part I, those marked by a cross from the results of Part II. The long straight portions of the curve cannot be regarded as quite accurate, for the following reasons:—That portion which lies close to the vertical (pressure) axis refers to the extremely short spark-lengths of Part I, in measuring which an error in the screw-reading or in the zero adjustment might be a considerable fraction of the

FIG. 8.



whole distance, and that portion which lies close to the horizontal (or spark-length) axis refers to the low pressures of Part II, in measuring which a small error might again make considerable difference. These portions apart, it will be noticed that the more curved portion of the curve is a fairly close approximation to a rectangular hyperbola, as may be seen from the following table:—

Pressure, mm. of mercury.	Spark-length, thousandths of an inch.	Product.
60	3·8	228
50	4·8	240
40	6·4	256
30	8·8	264
20	12·8	256
10	26·5	265

The portions of the curve more distant from the origin show considerable divergence from the hyperbola, though perhaps, as has been pointed out, not more than might be due to errors of observation.

The appearance of the spark-discharge changed with the pressure in the usual manner. At ordinary pressures the discharge was a bright white spark, gradually passing, as the pressure was lowered, into a purplish glow. In the case of the flat and convex discs of Part I, the purple discharge, at first central and narrow, gradually spread out and at low pressures filled the space between the plates. The appearance of the spark could not, however, be carefully examined, as the circuit was always broken as soon as discharge occurred in order to avoid damage to the surfaces of the plates. When these were examined by breathing on them after a number of sparks had passed at low pressures, it was always found that the part of the surfaces most affected by the discharge was an annulus at some little distance from the centre. With the flat plates used in Part II, the discharge was generally diffused over the space between the plates.

Observations of spark discharge between plates in air at different pressures have been made by Warren de la Rue and H. Müller* and by Macfarlane.† Their results do not present the features to which attention has been called in the present paper, nor could they do so, as the curves for potential difference and pressure for the spark-lengths they examined would show no evidence of a minimum potential difference at the lowest pressures they considered. In both cases the spark-length was about 0·13 inch, and the lowest pressure 20 mm. of mercury.

In conclusion, I have to express my thanks to Professor J. J. Thomson, to whom I have been indebted for advice and suggestions at every point of this investigation.

VII. "Electro-chemical Effects on Magnetising Iron. Part IV." By Thomas Andrews, F.R.S., M.Inst.C.E. Received May 16, 1892.

Influence of Magnetisation on Corrosion of Steel.

In connexion with, and during the progress of, the electrical portion of my research on "Electro-chemical Effects on Magnetising Iron," Parts I, II, and III ('Roy. Soc. Proc.,' vols. 42, 44, and 46), numerous gravimetric experiments were conducted, with the object of investigating the influence of magnetisation on the corrosion of iron and steel. I selected as the corrosive fluid a solution of cupric chloride, being partly guided in this choice by the results obtained with solutions of this salt in the electrical portion of the investigation.

* 'Phil. Trans.,' vol. 171, pp. 75—82, 1880.

† 'Trans. Roy. Soc. Edinb.,' vol. 28, p. 642, 1878.

The action of this salt on iron and steel is also very powerful, and ensures freedom from the disturbing influences of violent effervescence. Moreover, the results obtained with a copper solution of this nature afforded an opportunity of simultaneously obtaining an indication of the influence of magnetisation on the electrolytic deposition of copper from its solutions on iron and steel. The investigation was conducted as follows: the steel bars employed were of the lengths and diameters given in Table I, each pair being cut adjacently from a long finely-polished rod, so that the bars were as near as practicable alike in general composition and structure. For every set of experiments one of the steel bars was magnetised, the other being retained in its unmagnetised state. The bars were each weighed on the balance, and afterwards each bar was immersed in an equal quantity of cupric chloride solution, in separate beakers, a considerable distance apart (see fig. 1); the two beakers were of the same diameter and fluid capacity; the bars were placed in the beakers in the position shown in fig. 1. On the completion of the periods of immersion stated in

FIG. 1.

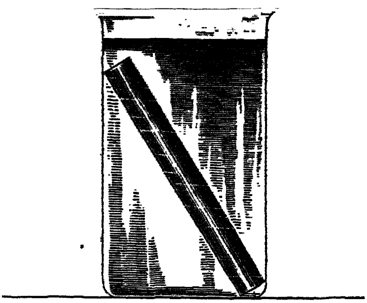


Table I, each bar was taken out, and on removal from the solution the electro-deposited copper was mostly found surrounding the steel bar in the form of a compact hollow pipe or cylinder; this was easily drawn off from the remains of the steel bar, which formed a kind of core within. The rods were carefully washed and cleansed from any loosely adherent copper and carbonaceous deposit, dried, and then weighed. A new pair of finely polished steel bars, one magnetised, the other unmagnetised, was prepared in the above-named manner for every experiment. The results obtained are stated in Table I.

The cupric chloride solution was prepared as follows: 4 ounces of crystallised cupric chloride were dissolved in 20 fluid ounces of water, and each steel bar was immersed as previously described in an equal quantity of this solution. A fresh solution of CuCl was prepared for every set of observations, and the methods resorted to, with manipu-

Table I.—Influence of Magnetisation on the Corrosion of Steel.

Size of bars. Length.	Size of bars. Diameter.	Time of exposure in cupric chloride solution.		Quantity of cupric chloride solution.	Loss in weight of unmagnetised steel bar.	Loss in weight of magnetised steel bar.	Increased loss in weight of the magnetised steel bar.
in.	in.	h.	m.		grains.	grains.	per cent.
4½	0·296	19	30	10 fluid ozs.	247·00	260·00	5·27
3	0·296	24	0	10 ,,	153·00	167·40	9·41
3	0·296	24	0	10 ,,	179·30	191·30	6·69
3	0·296	24	0	10 ,,	241·30	247·90	2·73
3	0·296	24	0	10 ,,	247·60	253·60	2·42
3	0·260	12	0	10 ,,	96·70	98·94	2·32
3	0·296	12	0	10 ,,	112·91	122·85	8·80
3	0·296	24	0	4000 fluid grs.	196·08	212·60	8·43
3	0·296	24	0	4000 ,,	179·80	187·34	4·19
3	0·296	24	0	4000 ,,	206·38	206·85	0·23
3	0·296	24	0	4000 ,,	189·03	190·54	0·80
4	0·250	24	0	10 fluid ozs.	233·43	237·39	1·70
4	0·250	24	0	10 ,,	221·26	227·46	2·80
4	0·250	24	0	10 ,,	219·53	222·14	1·19
4	0·250	24	0	10 ,,	229·40	230·22	0·36
4	0·260	24	0	10 ,,	224·13	228·72	2·05
4½	0·301	24	0	10 ,,	236·74	237·19	0·19
4½	0·301	15	35	10 ,,	215·32	219·59	1·98
4	0·260	18	30	10 ,,	210·54	215·30	2·26
4½	0·301	15	0	10 ,,	203·64	208·06	2·17
4	0·260	14	15	10 ,,	187·37	187·99	0·33
4½	0·301	6	0	4300 fluid grs.	165·04	173·10	4·88
4½	0·301	6	30	4200 ,,	164·17	171·99	4·76
4½	0·301	6	3	4200 ,,	164·92	169·59	2·83
4½	0·301	6	0	4200 ,,	151·13	152·24	0·73
4½	0·301	6	0	4200 ,,	211·50	221·37	4·67
4½	0·301	13	45	4200 ,,	259·53	262·21	1·03
4	0·260	13	0	4200 ,,	216·62	217·08	0·21
						Average.............	3·05

The temperature of the laboratory averaged about 55° F. during the experiments. At the end of each experiment the copper solution wa tested with a bit of bright iron to ascertain that it was not exhausted, and in every case copper was instantly deposited.

ERRATA. (VOL. 52.)

Page 117. *For* [Plate 5] *read* [Plate 1].

381, lines 20—21. *Delete* the words "Such interpolation has not been usual in the operations of the Indian Survey, and."

lative care, ensured exactitude for purposes of comparison between the behaviour of the magnetised and unmagnetised steel bars in the corrosive fluid.

An average of the twenty-nine experiments in Table I indicates an increase of corrosion in the steel due to magnetic influence of about 3 per cent. under the conditions of experimentation.

The steel bars were not highly magnetised, and I purposely exposed them to the action of the solution for somewhat long periods in the present experiments, as thereby, perhaps, affording a better indication of the influence of magnetisation on general corrosion, the almost immediate effect of magnetisation on the corrosion of steel having been demonstrated in the electrical observations of Part II.

It is probable that the deviation in the individual results of Table I, in the extent of the loss by corrosion, may be attributed to variation in the extent to which the several bars were magnetised. The results recorded in the present paper indicate that magnetisation exerts an effect, though small, on the extent of the corrosive action of copper salts on iron and steel. This is probably owing to the local currents, set up by magnetisation between the polar and central portions of the bars, inducing somewhat greater chemical action.

In some of the experiments with the copper solution which contained the more highly magnetised bars, the copper solution was of a perceptibly lighter colour towards the end of an experiment when compared with the colour of the copper solution containing the unmagnetised steel bar.

VIII. "Note on the Spectra of the Flames of some Metallic Compounds." By G. D. Liveing, M.A., F.R.S., Professor of Chemistry, and J. Dewar, LL.D., F.R.S., Jacksonian Professor, University of Cambridge. Received June 3, 1892.

[Plate 5.]

A study of the spectra of flames offers many points of interest. It is long since A. Mitscherlich (Poggendorff's 'Annalen,' vol. 116, p. 499; vol. 121, p. 459) showed that the spectra of flames are, for the most part, those of compounds of the elements present, and contain comparatively few rays proceeding directly from the elements themselves. But there are many questions still undecided. For example, it is not known whether the vibrations which give the spectra of compounds in flames are those which the molecules of the compounds in question would assume under the action of a high temperature alone, or whether they are not vibrations of a different order, arising during

chemical changes, and deriving their energy directly from the chemical energy of the interacting substances. When the absorption spectrum of a compound is observed to correspond with its emission spectrum in a flame, we may infer that the vibrations are those which the compound molecule assumes when sufficiently heated. But there are not many cases in which this has been observed. We have observed it in the case of cyanogen ('Roy. Soc. Proc.,' vol. 44, p. 247, note), but we are not certain of any other case. The difference between the spectrum of the base of a flame and that of the upper part, observed in many flames, lends support to the supposition that there are rays which originate in the chemical change, perhaps occurring in the molecules which are in intermediate stages of the change, and not assumed by the molecules which are the final product, even when intensely heated in the upper part of the flame. The fact that the same rays which are seen in the base of a flame may be sometimes generated by electric discharges in the gases which are burnt in the flame, or in their products of combustion, is not at all inconsistent with this supposition, for such discharges certainly have electrolytic effects, and may very well give rise to molecules in the intermediate stages between one state of chemical combination and another.* The flames of substances, such as the organo-metallic compounds, into which metals enter as chemical ingredients, have not hitherto, so far as we know, been observed, and it is to two such flames that these notes refer.

Spectrum of the Flame of Nickel-Carbonyl.

The remarkable compound of nickel and carbonic oxide, $Ni(CO)_4$, discovered by Mr. Mond, burns in air with a luminous, smoky flame, and the spectrum it emits appears to be a continuous one. When the vapour is burnt in oxygen instead of in atmospheric air, the spectrum still appears to be quite continuous; in fact, such a spectrum as carbonic oxide, without any nickel, gives under similar circumstances. This, however, is only in appearance, because the brightness of the continuous spectrum overpowers the feebler bands and lines which belong to the flame of the nickel compound. These bands and lines come out when the vapour of the nickel compound is diluted with a

* It is sometimes assumed in books on chemistry that the atoms which form a chemical compound can never be in an intermediate state between complete separation and complete combination. So inconceivable an assumption would hardly have been made except to support a theory, but it has nevertheless obtained a certain currency. It is supported by no fact and no analogy. Two atoms which are within the spheres of each other's influence, but have not yet reached the state of relative tranquillity which we recognise as chemical combination, may very conceivably be the seat of very violent agitation and vibratory motions, which cease when they are actually combined.

good deal of hydrogen. We have employed two methods of making such a mixture. The first was by passing a stream of dry hydrogen, mixed with carbonic oxide, over reduced nickel in a glass tube, and burning the issuing gas in a double jet with oxygen either outside or inside the burning gas. The nickel was freshly reduced at a gentle heat with hydrogen, and allowed to cool in carbonic oxide. When quite cold the stream of mixed hydrogen and carbonic oxide was found to take up quite enough nickel at the temperature of the room, and would continue to do so for some hours. After a time, however, the nickel required to be again warmed in a current of hydrogen, when some water was given off, and the metal recovered its sensitiveness. Another plan was to pass a stream of hydrogen through a U-tube containing a little of the liquid nickel compound in the bend. The result was the same in each case, but the proportion of vapour of the compound was more easily varied (by simply varying the proportion of carbonic oxide in the stream of gas) in the former method. The mixed gas and vapour burnt in air with a smoky flame, but in a full supply of oxygen with a bright yellowish-green flame without visible smoke. The first jet we used was of platinum, but nickel-carbonyl deposits nickel at a red heat, so that the platinum soon became coated with a thick deposit of nickel, which choked the orifices. This nickel adhered so closely to the platinum that it could not well be removed mechanically, and had to be dissolved off. We found it, therefore, more convenient to use a jet made of a piece of porcelain tube, about 1 cm. in diameter, with a narrow porcelain tube, fitted by means of a cork, in the axis of the wider tube. The mixed gas and vapour were passed either through the inner or through the outer tube, and oxygen through the other. The porcelain being a bad conductor, no nickel was deposited on it, except close to the orifice, whence it could be easily removed mechanically without disturbing the apparatus. The porcelain, of course, added some lines to the spectrum, but these were easily detected. In fact, we noticed only the lines of sodium, calcium, and lithium.

The spectrum of the flame of the nickel-carbonyl thus diluted consists of two parts: (1) the spectrum of the main body of the green flame, (2) that of the base of the flame when the oxygen is outside, and of the surface of the small inner cone when the oxygen is inside, the flame.

The spectrum of the main body of the flame consists of a series of shaded bands, brightest in the green, but extending on the red side beyond the red line of lithium, and on the violet side well into the blue, though with rapidly diminishing distinctness. These bands have their sharp bright edges on the more refrangible side, that is, they are turned in the opposite direction to the bands produced by electric

discharges in carbonic oxide at low pressure. The positions of the bright edges of the bands in the flame of the nickel-carbonyl have some correspondence with those of the bands produced by electric discharges in carbonic oxide, but it is not a very close one, and may be only accidental. With the dispersion employed, which gives a difference of deviation of 2° 52′ between D and F, there was no sign of a resolution of these bands into lines. The brightest bands had, however, their more refrangible edges pretty sharply defined, while the less bright bands, especially those in the blue, were very hazy. A certain amount of continuous spectrum, of course, overlay the bands, and made them somewhat less distinct. Photographs show that this continuous spectrum continues as far as λ 3500, but fading sensibly from λ 4200 onwards. The photographs do not show any extension of the bands beyond the blue.

Besides the bands, a few lines, but only a few, in the visible part of the spectrum, extend into the upper part of the flame. Of these few only one is a known line of nickel; it is the green line λ 5476. This was also the only line of nickel which we observed in the visible part of the spectrum in explosions of hydrogen and oxygen in a nickel-lined tube ('Roy. Soc. Proc.,' vol. 36, p. 475).

In the ultra-violet part of the spectrum of the flame a great number of nickel lines were photographed; indeed, by far the greater part of the lines of nickel found by us in the arc ('Phil. Trans.,' vol. 179 (1888), A, p. 247) between λ 3972 and λ 2943·5. In this case also there is a close correspondence between the spectra of the flame and of the explosions, except that the lines of the flame are much more numerous than those recorded of the explosions. This difference, however, is probably due to the much shorter exposure of the photographs of explosions. Although the photographs show lines as high as λ 2943, the lines in this region are very faint, and gradually die out in proceeding from the less to the more refrangible side of the spectrum. In the region about L, M, and N the lines are very strong, so that it is for rays of those rates of vibration that nickel is most sensitive at the temperature of the flame.

Turning now to the base of the flame, we find a great number of lines, of which most extend but a short distance from the bottom of the flame. They form two principal groups, one in the orange and red, and the other in the citron and yellow. These lines are for the most partly sharply defined, and in the more refrangible parts of each group very fine and closely set. They are probably channellings following Rydberg's law, and somewhat confused by overlapping. The diagram indicates the strongest of these lines, as they appear on the background of shaded bands in the flame. It is drawn to a scale of oscillation frequencies.

None of these lines appear to be nickel lines, and, as they are limited

to the base of the flame, they cannot be ascribed to any of the final products of the combustion, such as nickel oxide, but must be due either to the as yet unaltered molecules of nickel carbonyl, or to some molecules intermediate between that compound and the products of combustion which have only a transitory existence, and may perhaps have a transitory agitation of a particular kind imparted to them by the chemical energy which changes its form in the combustion.

The following tables give the approximate oscillation frequencies of the edges of the principal shaded bands, and of the lines seen at the base of the flame, but the numbers are only approximate.

Oscillation Frequencies of Edges of Shaded Bands.

1496	1692	1933	2146
1521	1752	1960	2172
1577	1808	2052	2199
1594	1849	2107	2226
1635			

Oscillation Frequencies of Lines in the Base of the Flame.

1497	1582	1622	1721
1506	group of very	1627 *l*	1727 *l*
1509	closely set lines	1631 *l*	1732 *l*
1514	1586	1651	1735
1518	1593	1656	1738
1521	1595	1663	1741 *l*
1526	1596	1667	1742
1543	1598	1671	1745
1547	1599	1673	1746
1549	1600	1682	1747
1555	1602	1686	1753
1560	1604	1690 *l*	1806
1563	1607	1706	1809
1572	1612	1712	1827
1575	1615	1714	1833
1578	1616	1716	1879
1580	1618		

The six numbers in the above table to which an *l* is added correspond to lines which extend into the upper part of the flame.

Table of Wave-lengths of Nickel Lines photographed from the Flame.

2943·5	3183·8	3390·4	3561·1
2981 2	3194 9	3392·4	3565 7
2983·6	3196·6	3409 0	3571·2
2992·2	3201·5	3413·4	3587·2
2994·1	3221·1	3413 8	3601·4
3002·1	3224·6	3423·1	3609·8
3003·2	3226·3	3433·0	3612·1
3011·5	3232·6	3436·7	3618·8
3018·8	3234·2	3445·7	3624·1
3031·4	3242·6	3452·3	3663·4
3037·5	3247·8	3457·8	3669·7
3044·5	3250·1	3461·1	3673·4
3050·4	3270·6	3466·8	3687·6
3053·9	3282·2	3468·9	3694·6
3057·2	3285·0	3470·8	3721·6
3064·2	3315·1	3483·1	3736·1
*3080·3 ?	3319·7	3485·2	3737·0
3096·6	3321·6	3492·3	3745·0
3098·6	3361·0	3500·0	3775·0
3101·1	3365·5	3509·7	3783 0
3101·4	3367·2	3514·4	3791·0
3105·0	3368·9	3519·1	3806·6
3113·7	3371·3	3523·9	3831·7
3133·6	3373·6	3527·1	3857·8
3145·5	3380·0	3547·5	3972 0

The more refrangible lines in the foregoing table were very faintly depicted on the photographic plate, and it is possible that a more lengthened exposure than the fifteen minutes, which we employed in the region where the lines were faint, would have brought out more lines. The continuous spectrum of the limelight extends some distance further than the most refrangible of these nickel lines.

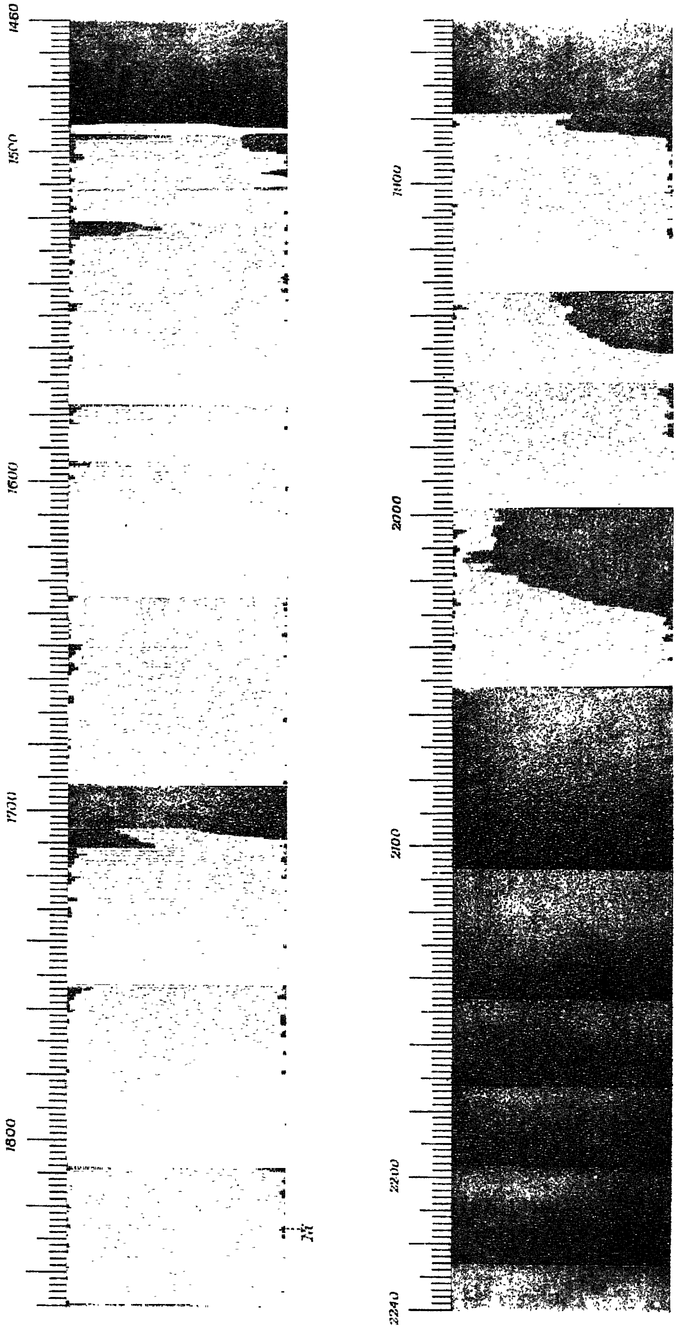
Flame of Zinc Ethide.

Zinc ethide burning undiluted produces so much continuous spectrum as to overpower any special rays. But by passing a stream of hydrogen through a bent tube containing zinc ethide, and burning the mixed gas and vapour in oxygen, as we did the nickel-carbonyl,

* A query is placed against this number because the water spectrum is so strong at this point that we cannot certainly distinguish the nickel line. There is no other reason for doubting its presence.

Spectrum of flame of Ni $(CO)^4$ + H burning in Oxygen.

Scale of Oscillation frequencies.



West Newman lith

we reduced the continuous spectrum sufficiently to enable us to observe any fairly strong rays which might be peculiar to the flame. In the visible part of the spectrum the three well-known rays of zinc in the blue λ 4812, 4721, and 4681 were easily seen. Photographs of the more refrangible part of the spectrum showed no trace of the ultra-violet lines of zinc; no more, in fact, than the flames of hydrocarbons usually show. Nor did there appear to be any rays from the base of the flame other than those seen in hydrocarbon flames in general. In our observations on explosions (*loc. cit.*) we did not find that a zinc lining to the tube in which the oxy-hydrogen gas was exploded brought out any zinc line, either in the visible or ultra-violet part of the spectrum. The flame of the compound containing zinc chemically combined may be supposed to give the rays of zinc more readily than the exploding gases, which merely take up the metal mechanically. But the flame does not, in either case, seem hot enough to develop the ultra-violet rays, though these are very strongly developed in the arc.

IX. "Preliminary Note on the Pressure developed by some New Explosives." By Captain NOBLE, C.B., F.R.S. Received June 10, 1892.

For a considerable time I have, with the assistance of Sir F. Abel and Professor Dewar, been engaged in researches upon the new explosives which during the last few years have attracted so much attention, and which apparently are destined to do much in developing the power of modern artillery.

From the nature of these researches and the considerable scale upon which they have to be conducted, as well as from certain difficulties which have manifested themselves, I am not at present in a position to submit to the Royal Society the results of these experiments; but, as one particular portion throws light upon a question of considerable importance, I propose very shortly to give the results at which I have arrived, leaving fuller details for a subsequent communication.

Artillerists of all nations are pretty well agreed that, save under exceptional circumstances, the maximum working pressure in a gun should not exceed 17 tons per square inch or, say 2500 atmospheres. The reasons for this limitation are weighty, but I need not here discuss them. Now, taking cordite and pebble powder as illustrations, since we can, even in guns not designed to fire the former explosive, obtain with the same maximum pressure, energies higher than those obtained with pebble powder by nearly 50 per cent., it is obvious that

this extra energy must be obtained from the development of higher pressures in the forward portions of the guns, and it naturally became a question of considerable importance to determine over what surface these higher pressures extended, and to ascertain if they in any serious degree affected the safety of the chase.

At Woolwich, to settle this point, certain guns were prepared in which crusher gauges were placed at various points along the bore, and results were obtained to which I shall presently more particularly allude; but, considerable doubt having been thrown on the reliability of these crusher gauges, I considered it desirable in a matter of so great importance to ascertain the pressures by altogether independent means, and thus either confirm the crusher-gauge results, or, if the two sets of results should prove to be not altogether in accordance, to throw some light upon the causes of such discrepancies as might exist.

The crusher gauge is, to those who interest themselves with such subjects, so well known, that I shall not attempt here to describe it, and I will only say that I have very great confidence in the accuracy of its results when properly used. Personally I have during the last twenty-five years made many thousand observations with these gauges, and when properly prepared and judiciously used, not only have I found their results accordant *inter se*, but I have by totally different determinations corroborated their accuracy. But I have always held that this gauge and all similar gauges will cease to be either reliable or accurate if there be any probability of the products of explosion being projected into the gauge at a high velocity, the energy stored up in such products being impressed on the gauge in the form of pressure, and this contingency might and does arise either when the gauge is placed in the forward part of a gun, where necessarily the products are in rapid motion, or in the case of the detonation of a high explosive; but, as I have gone pretty fully into this question elsewhere, I need not here pursue the subject further.

The crusher-gauge determinations for cordite, made at Woolwich for the Explosives Committee, under the presidency of Sir F. Abel, having been made in a 4·7-inch quick-firing gun, I arranged a similar gun in such a manner that I was able to obtain a curve determined from the time at which the projectile passed sixteen points arranged along the bore. From this curve, by methods I have elsewhere described, the curve giving the velocity at all points of the bore can be deduced, and from the curve of velocity the pressures generating these velocities can also be deduced.

For the particular purpose of this investigation it was desirable to compare the pressures of different explosives, and the present note gives the result of four explosives differing widely in nature and in composition.

The explosives used were as follows:—

a. Ordinary pebble powder of the Service. A charge of 12 lbs. was used; this charge gave rise to a mean pressure of 15·9 tons per square inch (maximum, 16·8; minimum, 14·9), or a mean of 2424 atmospheres (maximum, 2566; minimum, 2277) as determined by the crusher gauge in the powder chamber. It gave to a 45-lb. projectile a mean muzzle velocity of 1839 ft.-secs., thus developing a muzzle energy of 1055 ft.-tons. A gramme of pebble powder at a temperature of 0° C. and a barometric pressure of 760 mm. generates 280 c.c. of permanent gas, and develops 720 gramme units of heat.

b. Amide powder, consisting of 40 per cent. of potassic nitrate, 38 per cent. of ammonia nitrate, and 22 per cent. of charcoal. The charge in this case was 10 lbs. 8 ozs., and the mean crusher gauge pressure was 15·3 tons per square inch (maximum, 16·4; minimum, 14·2), or a mean of 2332 atmospheres (maximum, 2500; minimum, 2165); the muzzle velocity with the same projectile was 2036 ft.-secs., and the muzzle energy 1293 ft.-tons. A gramme of amide powder generates 400 c.c. of permanent gases, and develops 821 units of heat.

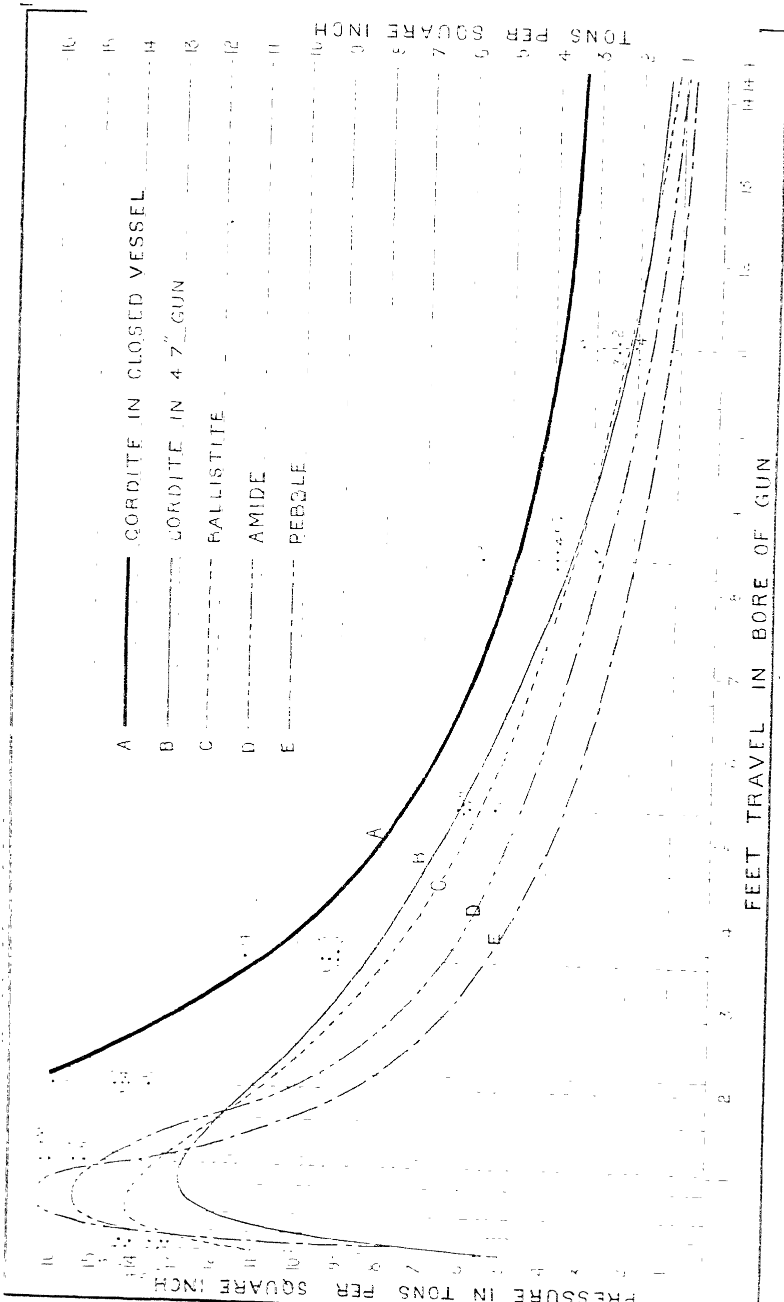
c. Ballistite. With this true smokeless powder the charge was reduced to 5 lbs. 8 ozs., the sides of the cubes being 0·2 inch. The mean crusher-gauge pressure was 14·3 tons per square inch (maximum, 14·5; minimum, 14·1), or a mean of 2180 atmospheres (maximum, 2210; minimum, 2142). The muzzle velocity was 2140 ft.-secs., and the muzzle energy 1429 ft.-tons. A gramme of ballistite generates 615 c.c. of permanent gases, and gives rise to 1365 gramme units of heat.

d. With the fourth explosive, Cordite, a charge of 5 lbs. 10 ozs. of 0·2 inch diameter was fired. The mean chamber crusher-gauge pressure was 13·3 tons per square inch (maximum, 13·6; minimum, 12·9), or a mean of 2027 atmospheres (maximum, 2070; minimum, 1970). The muzzle velocity was 2146 ft.-secs., and the muzzle energy 1437 ft.-tons. A gramme of cordite generates 700 c.c. of permanent gases at 0° C. and 760 mm. of barometric pressure. The quantity of heat developed is 1260 gramme units. In the case of this explosive, as well as in that of ballistite, a considerable quantity of aqueous vapour has to be added to the permanent gases.

The results of these observations are graphically given in the figure. The ordinates show both the positions at which the pressures were determined, and the magnitudes of these pressures. On the axis of abscissæ is shown the travel of the shot in feet.

Each curve is deduced from the mean of three* complete rounds,

* Since these experiments were carried out, a second set of induction coils has been added, so that a single round gives simultaneously the times both at the breech and muzzle plugs.


A CORDITE IN CLOSED VESSEL
B CORDITE IN 4 7" GUN
C BALLISTITE
D AMIDE
E PEBBLE
TONS PER SQUARE INCH
PRESSURE IN TONS PER SQUARE INCH
FEET TRAVEL IN BORE OF GUN

that is to say, three rounds with the breech and three with the muzzle plugs, or six in all.

In the calculation of the pressures, it is assumed that before and after the complete ignition of the explosive the pressures will uniformly increase and then uniformly decrease, following the laws regulating the relation between the pressure and volume when permanent gases are permitted to expand with production of work.

It is in the highest degree improbable that in any experiment is this assumption strictly true. In the case of "brisante" powders, and of high explosives which can be detonated, we know that even in moderately-sized chambers there are wide variations in pressure; and even when there is comparatively slow combustion, as with ballistite and cordite, it appears probable that the generation of a considerable quantity of the gases may take place in different portions of the bore, giving rise to a corresponding difference in the pressure, and it may be that a portion of the discrepancies of the crusher gauge when placed in forward positions in the bore, to which I shall presently allude, are due to this cause.

But such irregularities would not very seriously alter the curves shown in the figure. The areas included between the final ordinate, the curve, and the axis of abscissæ, being the total energy impressed on the projectile, would of course remain unaltered; but the curves, instead of the regular figure there shown, would have a wavy outline, and would show several maxima and minima. These irregularities of pressure are of no appreciable importance when the strength of the gun, in a radial direction, is considered.

From the same plate can be at once obtained the pressures for the four explosives at any point of the bore; but for the purpose of applying these results to other guns I give in the annexed table for different densities of the products of explosion (1) the pressure which has been determined in a closed vessel, (2) the pressure at the same density, which has been found to exist in the bore of the 4·7-inch gun where the gases have been expanded, doing work on the projectile.

Comparison of Pressures in Closed Vessel with those in 4·7-inch Q.F. Gun.

Pebble powder.			Amide powder.		
Density.	Pressure.		Density.	Pressure.	
	In closed vessel.*	In gun.		In closed vessel.	In gun.
0·65	17·68	15·82			
0·60	15·55	14·55			
0·55	13·62	11·84	0·55	25·50	15·50
0·50	11·85	9·31	0·50	22·00	13·75
0·45	10·23	8·12	0·45	19·10	12·10
0·40	8·73	7·00	0·40	16·50	10·00
0·35	7·35	6·11	0·35	14·00	8·34
0·30	6·07	5·21	0·30	11·90	7·05
0·25	4·88	4·20	0·25	9·80	5·88
0·20	3·77	2·95	0·20	7·75	4·55
0·15	2·73	1·82	0·15	5·00	3·10
0·10	1·76	0·70	0·10	3·55	1·32
Ballistite.			Cordite.		
0·26	20·80	13·45	0·26	21·75	12·50
0·24	19·00	12·70	0·24	19·80	11·95
0·22	17·10	11·83	0·22	17·90	11·28
0·20	15·30	10·77	0·20	16·00	10·60
0·18	13·40	9·75	0·18	14·20	9·85
0·16	11·70	8·70	0·16	12·30	9·10
0·14	10·00	7·50	0·14	10·50	8·16
0·12	8·40	6·52	0·12	8·70	7·08
0·10	6·60	5·29	0·10	7·10	5·70
0·08	5·00	4·08	0·08	5·40	4·01
0·06	3·50	2·54	0·06	3·80	2·33
0·05	2·80	1·64	0·05	3·00	1·57

It must be understood that the differences existing between the close-vessel pressures and the pressures observed in the bore at the higher gravimetric densities are, in great measure, due to the explosive at these densities not being fully consumed. An examination of the results obtained would lead to the conclusion that with the particular size of cordite employed tolerably complete combustion cannot be assumed to have taken place until the projectile has travelled some 6 or 7 feet through the bore. Indeed, if the size of cordite fired in this gun be even slightly increased, a portion of the cordite is blown from the muzzle unburned, and it is one of the most

* 'Phil. Trans.,' Part I, 1875, p. 129.

striking proofs of the regular combustion of the explosive that in all such recovered cordite the diameter is so uniformly decreased that it might readily be mistaken for newly-manufactured cordite of smaller dimensions.

I now make a comparison between the pressures given by cordite as shown on curve B (see fig.) and those obtained from crusher gauges. To facilitate this comparison I have added to the pressure curves I have described a curve showing the pressures developed by cordite when fired in a close vessel, and I have further added the results of five rounds of cordite fired for the Explosives Committee in the crusher-gauge gun, the pressure of each individual round at each point of observation being indicated.

The sample of cordite used in these experiments was not of the same make as that employed in my own. The pressures given on the axis of y denote those taken in the powder chamber, and are comparable with the crusher-gauge pressures I have given as derived from my own experiments. It will be observed that the mean chamber pressure indicated is two- or three-tenths of a ton higher than that I obtained; but it will be further observed that if I attempt to draw a pressure curve through the mean of the crusher-gauge observation, such curve would indicate pressures far higher than are necessary and sufficient to develop the work impressed on the projectile.

Again, the pressures indicated after the projectile has moved 1 foot are about 2 tons per square inch higher than those observed in the powder chamber, and it will be further noticed that not only are these observations, at all events at certain points, considerably too high, but they exhibit in the forward part of the bore variations quite unknown when the pressures are taken in the powder chamber.

Thus, in the particular experiments I am discussing, the mean pressure in the powder chamber being about 13·5 tons, the extreme variation in the five rounds amounts only to about $1\frac{1}{3}$ tons per square inch, while the crusher gauge placed in the chase at a point about $8\frac{1}{2}$ feet from the seat of the shot, gave in the same number of rounds an extreme variation of 3 tons per square inch, the mean pressures being only about 4 tons, and it will further be noted that, while some of the rounds indicated pressures below those deduced by the method I have described, other rounds at the same point indicated pressures even exceeding those which would have existed under the same gravimetric densities in a close vessel. It may also be noted that from the crusher-gauge experiments, round 5 should have given the lowest muzzle energy of the series; as a matter of fact it gave the highest.

My conclusion, therefore, is that, although crusher gauges placed in the chase may, and doubtless do, give valuable comparative results, they cannot be relied on for absolute determinations, unless confirmed by observations altogether independent in their nature.

X. "The Reserve Proteid of the Asparagus Root." By S. H. Vines, F.R.S., and J. R. Green, M.A., F.L.S. Received June 10, 1892.

The object of research was the determination of the nature of the substance, presumably a proteid, from which is formed the asparagin which is so abundantly present in the young shoots of the plant in the spring. The researches were carried on during the springs of 1891 and 1892, and results have been obtained which are sufficiently definite for publication.

Microscopical Observations.—When a transverse section of a fresh root, taken from the plant while in a state of winter-rest, is mounted in alcohol on a slide, and is at once examined with the microscope, some of the parenchymatous cortical cells are seen to contain relatively large masses of irregular shape. When water is run under the cover-slip these masses at once dissolve. From their general appearance and their reaction with iodine these masses probably consist of some form of proteid.

Chemical Observations.

a. Watery Extract.—The watery extract was made by pounding the fresh root in a mortar with distilled water (generally 100 c.c. water to 100 grams root), and straining off and filtering the liquid. The resulting extract was faintly yellow, slightly opalescent, and feebly acid. On boiling it gave a dense precipitate, which gave a good xanthoproteic reaction, thus indicating its proteid nature. The liquid filtered off from the precipitate gave a faint xanthoproteic reaction. In some experiments the extract was carefully neutralised with ammonia to precipitate the phosphates, and then some common salt (NaCl) was added to throw down a substance of unknown nature but not a proteid, the presence of which was detected at an early stage in the investigation; the neutral liquid then filtered off gave only a turbidity on boiling; when the liquid was made slightly alkaline the same result was given; but in both cases when the extract was made faintly acid a good precipitate was obtained on boiling. The coagulation point was determined to be 71—73° C.

The watery extract gave a good xanthoproteic reaction; a fairly good reaction with Millon's reagent; no satisfactory reaction with acetic acid and potassic ferrocyanide or with potassic hydrate and copper sulphate.

When alcohol in excess was added there was a dense precipitate which at first was soluble again in water, but which became for the most part insoluble if allowed to remain for some time in contact

with the alcohol. This precipitate when suspended in water, after prolonged contact with alcohol, gave a good xanthoproteic reaction.

When the watery extract was allowed to drop into a tall jar containing distilled water, there was no precipitation or turbidity. Similarly, when some of the extract was diluted with fifteen times its volume of distilled water, and a stream of CO_2 was passed through the liquid for 1—6 hours, no turbidity was apparent.

When some of the extract was subjected to dialysis in a stream of running water for four days, and the dialysis afterwards continued for two days in large excess of distilled water, a finely granular precipitate was formed in the dialyser, which when filtered off gave no xanthoproteic reaction, whilst the filtrate still coagulated at 71° C.

The proteid was therefore soluble in distilled water, being thrown down neither by dilution nor dialysis.

Portions of the extract were shaken up to saturation for some days with crystals of the following neutral salts: NaCl, $MgSO_4$, $MgSO_4$ and Na_2SO_4, $(NH_4)_2SO_4$.

With NaCl there was a slight precipitation of the proteid, but the greater part of it remained in solution.

With $MgSO_4$ there was a more copious, but still incomplete, precipitation of the proteid: on saturating the filtrate with Na_2SO_4 no further precipitate fell.

With $(NH_4)_2SO_4$, when the saturation was continued for a month, the whole of the proteid was precipitated.

b. Salt Solution Extract.—This extract was prepared by using 10 per cent. solution of NaCl instead of distilled water, as in the former experiments. The reactions were generally the same as those given by the watery extract; but the undetermined substance, which is precipitable by salt, was present in much smaller quantities, and the precipitates of proteid were less bulky than those given by the watery extract.

Conclusions as to the Proteid.—It appears from the foregoing experiments that the root of the asparagus contains a single reserve proteid. Inasmuch as it is readily soluble in distilled water, it is essentially an albumin; at the same time, its reactions with neutral salts indicate a relationship to the globulins which is not manifested by the animal albumins. However, proteids other than globulins are precipitated on saturation of their solutions with neutral salts; thus Schäfer* and Halliburton† have both shown that serum-albumin is completely thrown down on double saturation with magnesium and sodium sulphates, and Kühne and Chittenden‡ have found that the

* 'Journal of Physiology,' vol. 3, 1882, p. 184.

† *Ibid.*, vol. 5, 1884, p 178.

‡ "Ueber Albumosen," 'Zeitschrift für Biologie,' xxii.

albumoses are precipitated by NaCl from their, in some cases neutral, in others faintly acid, solutions.

Besides the proteid we have found three undetermined substances in the extracts, neither of which is proteid. The first of these (1) is the substance which is present in considerable quantity in the watery extract, and which is precipitated on adding a small amount of salt.

(2.) When a portion of the first extract is freed from proteid by boiling, and then the filtrate poured into alcohol, a precipitate is formed, fairly copious but much less than that formed when the unboiled extract is similarly neutral, as described above. This remains soluble in water after prolonged action of the alcohol, but the solution gives no xanthoproteic reaction.

(3.) When the alcohol is evaporated to dryness a sticky residue is left, which also is soluble in water, and its solution gives a fair xanthoproteic reaction. This is not proteid, however, as it is soluble in alcohol. This substance can be extracted from the fresh extract by dialysing it in distilled water. On concentrating this dialysate a similar sticky residue is obtained. In several cases this brown, sticky mass deposited crystals of rounded form, much resembling in appearance the well-known aggregations of leucin. They were not leucin, however, as besides being soluble in cold alcohol they did not give the characteristic Scherer's reaction, nor did they form a compound with the acetates of lead or zinc. It is not improbable that this third undetermined substance may be allied to leucin, asparagin, &c., but our observations on it are as yet incomplete.

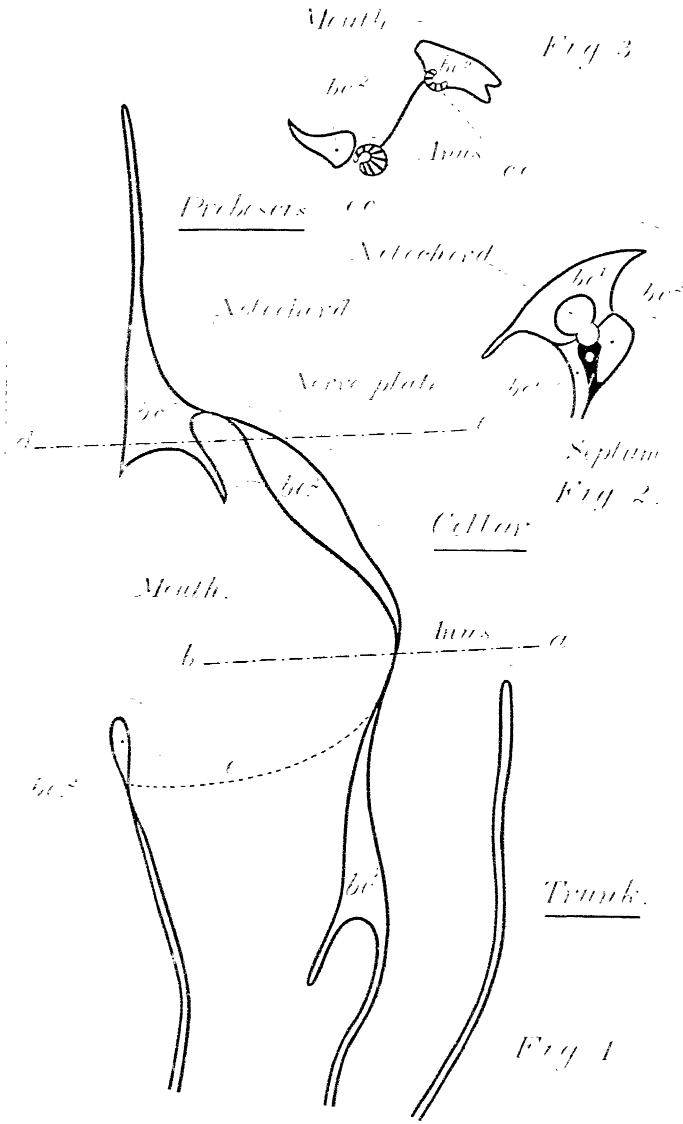
XI. "Note on the Structure of *Rhabdopleura*." By G. HERBERT FOWLER, B.A., Ph.D., Assistant in the Zoological Laboratory of University College, London. Communicated by Prof. W. F. R. WELDON, F.R.S. Received June 13, 1892.

The specimens investigated were attached to a colony of *Lophohelia*, obtained by the "Challenger" Expedition at Nightingale Island, from a depth of 100—150 fathoms. I owe to Mr. John Murray my thanks for his courtesy in allowing me to publish my notes on the structure of this interesting form, in which I hoped that the improved methods of microscopical research introduced in recent years might reveal points which had, perhaps, escaped the two observers to whose study of the living animal we owe our present knowledge of *Rhabdopleura*.

All the new anatomical features which I have been able to detect are in entire agreement with the structure of *Cephalodiscus; Rhabdopleura* may thus be taken to form a third member of Bateson's order, the Hemichordata. They are, briefly, as follows:—

The epistome corresponds to the proboscis of *Balanoglossus* and *Cephalodiscus*, and, like them, contains a portion of the cœlom, the anterior or præ-oral body cavity, completely shut off from the other portions of the cœlom.

The collar region (thorax of Lankester) contains the central section



of the cœlom, divided into right and left halves by median septa, each half communicating with the exterior by means of its own collar canal; on the posterior face of this cavity is an ectodermal thickening, which corresponds in position with the nerve-plate of *Cephalodiscus* and the nerve-tube of *Balanoglossus*. The collar cavities are continued upwards into the tentacles, and surround the mouth. From the pharynx a short diverticulum is given off upwards, which is continuous with a rod-like structure, apparently half cellular, half gelatinoid, which lies in the line along which the median posterior septum of the collar meets the nearly vertical septum between proboscis and collar cavities. It thus corresponds in origin, structure, and position with the notochord of *Cephalodiscus*.

The trunk contains the greater part of the alimentary canal. Its body cavity, as in the other Hemichordata, appears to be completely shut off from the paired cavity of the collar. The only part of the intestine calling for remark is a short semicircular diverticulum, which occurs also in *Cephalodiscus*.

The points in which *Rhabdopleura* differs from both the other Hemichordata are purely negative, viz., the absence of a proboscis pore or pores, and the absence of gill-slits; the points of agreement are so striking that it is impossible to separate the three organisms.

A more fully illustrated paper on the subject will shortly be published.

DESCRIPTION OF FIGURES.

FIG. 1.—Longitudinal section, taken slightly to one side of the middle line, so as to avoid the median septa of the collar region. The dotted line *e* marks the ventral limit of the collar.

FIG. 2.—Transverse section along the line *c—d* in fig. 1, through proboscis stalk and upper part of collar, and cutting the base of a tentacle of one side.

FIG. 3.—Transverse section along the line *a—b* in fig. 1, showing the external opening of one, the internal opening of the other, collar canal.

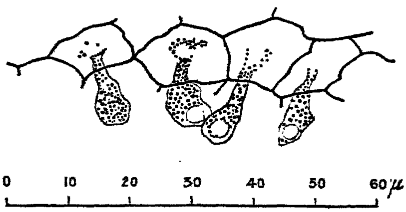
In all three figures the mesoderm is represented by a thick line, the ectoderm and endoderm by thin lines. The gelatinoid part of the notochord is marked by cross-hatching; its cellular part has not been distinguished from ectoderm or endoderm; $b.c.^1$, præ-oral cœlom, or proboscis cavity; $b.c.^2$, cœlom of the collar divided by the median septa into right and left halves; $b.c.^3$, cœlom of the trunk; *c.c.*, canals leading from the collar cavities to the exterior.

XII. "On the Flask-shaped Ectoderm and Spongoblasts in one of the Keratosa." By GEORGE BIDDER. Communicated by ADAM SEDGWICK, M.A., F.R.S. Received June 15, 1892.

In my "Note on Excretion in Sponges," published by the Society in the 'Proceedings' of this year, I said: "Both from my own observations on an *Aplysilla* (?) . . . . and from a study of

Schulze's detailed description . . . . particularly in *Euspongia*, I am persuaded that the ectoderm cells of the horny sponges are of the same form and character as those in the Homocœla." By a piece of good fortune I am now able to state that this is so. In a sponge found at Naples, which appears to me to correspond fairly with O. Schmidt's *Cacospongia scalaris*, the ectodermal elements are quite plainly to be seen even with a low power; indeed in the specimen examined their arrangement is more regular, invariable, and easy to observe than in any sponge that I have yet investigated.

FIG. 1.



Ectoderm Cells of *Cacospongia, sp.*

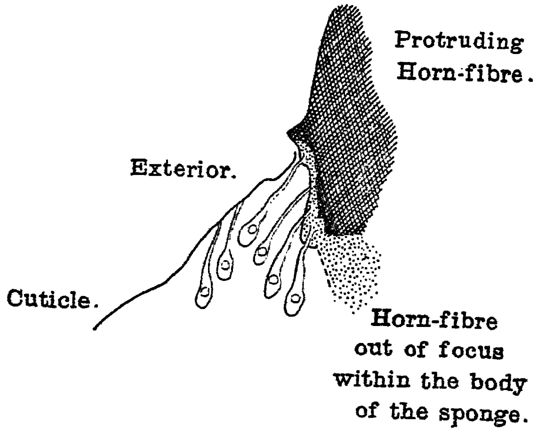
The cell-bodies are shown focussed through the silver-lines; the neck of each cell lies towards the upper part of the figure; it is at the same focus as the silver area, in the middle of which its granules lie on the outer surface.

Dilute osmic acid, followed by nitrate of potash and nitrate of silver.

The cells in question are of a flask-shaped form, very slender, elongated, and thin-necked. I have been able most successfully to demonstrate their relations to the silver areas (fig. 1), to prove without doubt that they open on the surface in the centre of those areas and not in any interstices between them, to prove also equally without doubt that each silver area has no nucleus connected with it except the one lying in the base of the pendent cell body ("subdermal gland-cell" of authors), and thus to justify completely the inability of Schulze and other trustworthy investigators to find nuclei at a more superficial level, where the "flat epithelium" was usually supposed to exist.

In this sponge the spongoblasts of the primary fibres form a continuous tissue with the ectoderm cells and resemble them indistinguishably both in form and character; a brief discussion of previous observation on this point will be found in the paper already quoted. It is nearly impossible—at least, for my inexperience—to give the effect of a three-dimensional preparation except by a most elaborate drawing, but the outlines given in figs. 2 and 3 will render some idea of the appearances on which I base this statement. The impression

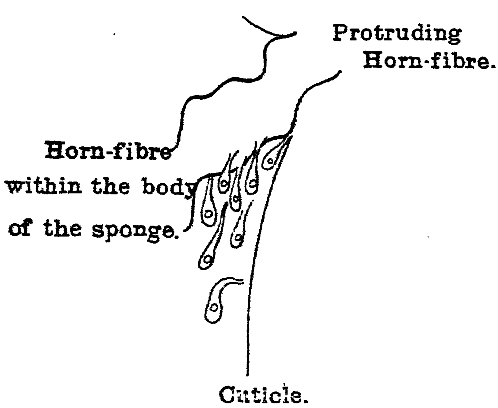
FIG. 2.



Ectoderm Cells at the apex of a conulus of *Cacospongia sp.* (Focus near the surface of the skin.)

The shading is diagrammatic; the dark shading indicates apparently naked horn-fibre, the light shading horn-fibre covered with living tissue.

FIG. 3.



Spongoblasts near the apex of a conulus of *Cacospongia sp.* (Focus near the profile of the fibre. Slightly smaller scale than fig. 2.)

Figs. 2 and 3 are from the same preparation, prepared with osmic acid, picro-carmine, and glycerine.

given is that the apex of a conulus is a locus of attraction for ecto-derm cells, and that the fibre is nothing more than the concentrated

cuticle of a large number of such cells poured out (in this sponge) around an intrusive foreign object.

I referred (*loc. cit.*), as if well known, to the "hexagonal markings of the horny fibre." I cannot, however, find any mention of them in the literature of the subject, except by Poléjaeff ('Keratosa,' p. 70). As he there describes them as an exceptional structure, and as I find them not nearly so easily to be made out in *Cacospongia* as in *Aplysilla*,* it is possible that they have hitherto escaped notice. They are fine black lines, having the exact appearance of the network on the surface of an epithelium, and I regard it as beyond doubt that they mark the lines of contact between the expanded secreting ends of the spongoblasts.

An interpretation of the histology of *Ascetta clathrus*, differing considerably from mine, appears in the 'Zoologischer Anzeiger,' No. 391, for this year. It would be under-valuing Mr. Minchin's researches to attempt serious comment on them until I have had the opportunity of re-examining my preparations in the light of his conclusions. How far his theory of a contractile ectoderm may be applicable to such an extreme form as that found in *Cacospongia* is a matter demanding the gravest consideration.†

The newly-established points on which we find ourselves in agreement are: (1) The pores of *Ascetta clathrus* are composed each of a single, nucleate, perforated cell; these cells have by various authors been mistaken for mesoderm cells. (2) The ectoderm is, in life, in a large part composed of cells which are roughly T-shaped in section, the nucleus lying at the base of the stalk of the T.

On a minor and not very novel point we are in agreement as to there being, at any rate commonly, at least one vacuole in the distal part of the collar cell, frequently containing shapeless particles. As to our differences, I am undecided whether Minchin's mesodermal "potato-shaped wandering cell, of greenish-yellow colour," is what I

* The determination of these sponges must be taken with reserve; the fear of angels to tread among Keratose genera makes me postpone as far as possible my first step! Both species are found in the Bay of Naples. The "*Cacospongia*" is firm, massive, somewhat incrusting, conulate, with conspicuous antler-like primary fibres, heavily cored with foreign bodies; the delicate secondaries show in their axis fine black spots; skin black, pulp white. "*Aplysilla*" corresponds in general description with Schmidt's *Aplysina aërophoba*, the yellow crust bearing vertical twig-like prominences, mostly supported by a single fibre; under unhealthy conditions, the living part of the prominences contracts up the fibre like a bud. In the axis of the fibre—at least in one specimen—I have observed a row of indubitable cells, amœboid in appearance; presumably they are what Lendenfeld has described as "spongoklasts."

† It is obvious that the presence, as in this sponge, of a fairly strong cuticle, even though it be "elastic" (Schulze), renders it much more difficult to assume that the form of the cells has been produced by their contraction. (July 15.—The continuity with the spongoblasts greatly increases the difficulty of such an hypothesis.)

call a Metschnikoff cell or not. I consider the natural form of active collar cells, in this as in other sponges, as being more separated in their upper parts than in his figures, a large part of the surface of the cell being washed by the water; this area behind the collar I have always regarded as the ingestive surface. Mr. Minchin believes that he has obtained evidence that the ectoderm cells really push their way to the endoderm; holding the views I do as to excretion, I shall be very glad if re-investigation causes me to admit this correction.

Apart from our common ground, I have made the indigo-carmine experiments with which the Society has been acquainted, and Minchin has made his valuable observations on the changes in form of the epithelium surrounding the oscula of *Ascetta clathrus*.* Whatever weight should be attached to these two factors in deciding the meaning and function of the flask-shaped epithelium, I wish to establish the undoubted presence of such a structure in *Cacospongia*, and presumably in all Porifera. Examining *Cacospongia*, and comparing Merejkovsky's figure of *Halisarca*, any one will be convinced that this is the true explanation of the puzzling appearances seen by various authors on the outer surface of other sponges—I would take as examples, among very many, Schulze on *Halisarca Dujardini* and *Chondrosia reniformis*, Poléjaeff on *Janthella*, Sollas on *Stelletta Normani* and *Isops Phlegræi*, Vosmaer on *Tentorium* (*Thecophora*) and the larva of *Myxilla*. Vosmaer's figure of *Myxilla* (fig. 8, Pl. XIV), unites with Metschnikoff's of the young *Halisarca*, to show that the flask-shaped cells appear at the earliest stages, and are probably formed by direct metamorphosis from the columnar epithelium of the free larva. On the other hand, the direct observations of flat epithelium on the outer surface are strikingly few.†

I hold that it may take rank as an established fact that in all groups of sponges the flask-shaped epithelium does occur. It must be recognised that the structure we are to expect to find on the exterior of any sponge is a zone of hyaline tissue traversed by necks which unite the centre of each silver area to the nucleus and cell-body pertaining to it. This, whatever be its physiological explanation, is the structure most commonly to be met with, and not a flat epithelium, such as clothes the canals.

And I believe that these cells are excretory, and identical with the spongoblasts.

* I should say that I can support the figure he gives as being an accurate representation of transitions in form of epithelial cells to be met with in all the Calcarea. I have hitherto ascribed to them an entirely different significance—that where the wall is thin, the special elongated flask-shaped form of cell is not required for excretion.

† The descriptions of flat epithelium—generally flagellate—by R. v. Lendenfeld are numerous and detailed. I attach no credit to them.

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XIII. "Contributions to the Anatomy of Fishes. II. The Air-Bladder and Weberian Ossicles in the Siluroid Fishes." By T. W. Bridge, M.A., Professor of Zoology in the Mason College, Birmingham, and A. C. Haddon, M.A., Professor of Zoology in the Royal College of Science, Dublin. Communicated by Professor Newton, F.R.S. Received June 9, 1892.

(Abstract.)

An abstract of the first part of this memoir, which dealt with the structure and relations of the air-bladder and Weber's ossicles in the Siluridæ, was published in the Proceedings of this Society three years ago ('Roy. Soc. Proc.,' vol. 46, 1889, pp. 309—328). The present contribution is a discussion of the physiology not only of this remarkable mechanism, but of the air-bladder in general.

There is a strong *a priori* probability that the Weberian mechanism is physiologically related to one of the several functions that have been ascribed to the auditory organ or to the air-bladder, but to which of them is a question by no means easy to answer. A preliminary difficulty to be encountered is the complex physiological character of the two organs, and, apart from our imperfect knowledge of the physiology of the several functions assigned to each, and especially in the case of the auditory organ of Fishes, a further difficulty is

afforded by the almost total absence of any experimental evidence directly bearing on the physiological significance of the Weberian ossicles; and while we desire to emphasise the danger of deducing function from facts of a purely anatomical nature, no other course has yet been adopted by previous writers on this subject, and at present is the only one open to us; consequently, any conclusions based upon data so frequently unreliable and misleading must partake rather of the nature of suggestions, and be accepted with considerable reserve. With the qualifications rendered necessary by these considerations, some light may possibly be thrown on this difficult problem by a careful inquiry as to how far the Weberian ossicles and the coadapted parts of the air-bladder and auditory organ are anatomically fitted to act as subsidiary or accessory structures in connection with any of the several functions assigned either to the air-bladder or auditory organ, while unsuited for association with others. By this means it may at least be possible to eliminate certain functions from any further consideration, and thereby considerably narrow the scope of future inquiry.

With this object we propose to discuss (I) how far the function of the Weberian mechanism is conditioned by the anatomical structure of the air-bladder and auditory organ, as well as by the character of the mechanism itself; (II) to which of the known functions of the air-bladder and auditory organ the Weberian ossicles are to be regarded as accessory structures; and (III) the utility of the mechanism to the Fish possessing it.

I. In all the Siluridæ normales the air-bladder may be regarded as consisting of two intercommunicating but physiologically distinct portions—a posterior, represented by the two lateral compartments, which is indistensible and inelastic, and always of greater internal capacity, and an anterior, which is always more or less elastic and expansible, but of less internal capacity than the former. The distensibility of the anterior chamber is, however, by no means uniform in all directions; on the contrary, the peculiar construction of the chamber and its intimate relations and connections with neighbouring skeletal structures render it absolutely inexpansible except laterally, that is, in a direction at right angles to its antero-posterior axis; and from the mode in which the fibres forming the lateral walls of the chamber converge in the dorsal wall, and become inserted into the crescentic processes of the tripodes, it becomes still more obvious that it is only by inward or outward bulgings of the lateral walls that variations in the internal condition of the air-bladder are able to set the Weberian ossicles in motion. It is scarcely necessary to point out that by this restriction of the expansion or contraction of the anterior chamber to movements of its lateral walls, the Weberian ossicles are rendered more suscep-

tible, and therefore capable of responding to smaller variations of internal gaseous tension by whatever cause produced than if the anterior chamber were equally elastic and expansible in all directions.

The increased delicacy of the Weberian mechanism in the Siluridæ normales, as compared with other Ostariophyseæ, is probably the cause of the extensive anchylosis of the anterior vertebræ, and their rigid articulation with the skull, for if flexibly articulated with one another and to the skull, so as to be able to participate in the lateral flexion of the vertebral column in ordinary locomotion, while at the same time the vertebræ and their processes retained their intimate relations with the air-bladder, the anterior chamber and Weberian ossicles could hardly fail to be affected by muscular compression. Hence, anchylosis of the anterior vertebræ becomes almost a necessity in the Siluridæ, if the Weberian mechanism is to remain unaffected by the more or less violent shocks produced by muscular contraction and relaxation. From a physiological point of view, it may be noted in connection with the Weberian ossicles, (*a*) that the anterior and crescentic processes of the tripodes are of approximately equal length, and hence the force and amplitude of all movements imparted to one extremity will be exactly reproduced, without augmentation, at the other; (*b*) the interposition of a lax, or at all events compressible, ligament (interossicular ligament) in the series of ossicles; and (*c*) the rudimentary and functionless condition of the intercalarium.

These and other facts suggest the conclusion that the Weberian apparatus is far better adapted to register the more forcible, even if more gradual, distensions or contractions of the anterior chamber, rather than slight or rapidly recurring vibrations of its lateral walls.

Finally, it may be affirmed that no differential action of the two auditory organs can possibly take place, at all events so far as impulses received through the Weberian mechanism are concerned, since the only channel through which any movement initiated in the fluids of the atrial cavities by the motion of the Weberian ossicles can reach them is the median, unpaired sinus endolymphaticus (the "sinus impar" of Weber); hence it must follow that each auditory organ will be affected by any such disturbances to an equal extent and at the same moment.

II. So far as the auditory organ is concerned, the functions of audition and equilibration or orientation have to be considered, and for the air-bladder those of phonation or sound production, respiration, accessory to audition, or its function may be purely hydrostatic.

Certain of these possible functions may be at once eliminated from any discussion as to the use of the Weberian mechanism. Equilibration may be dismissed, inasmuch as there can be no differential action of the two auditory organs. The absence of intrinsic muscular fibres

in the walls of the air-bladder, of extrinsic muscles in all but a few genera (Pimelodinæ), and of internal vibratory diaphragms, or other obviously vocal structures, is sufficient to prove that the air-bladder takes little or no part in this function, at all events by any of the ordinary methods known in other Fishes. The feeble vascularity of the air-bladder and the absence of any inspiratory or expiratory mechanisms are serious objections to its use as an ordinary respiratory organ.

In many Fishes the bladder appears to have a secondary relation to respiration by acting as a reservoir for the superabundance of oxygen introduced into the blood by the gills, which can be re-absorbed when required, but Moreau's experiments prove that those Fishes provided with a Weberian mechanism have a far less capacity for absorbing oxygen from the air-bladder than other Teleostei have under precisely similar conditions, and, further, that the capacity for oxygen absorption is always associated with the presence of retia mirabilia or vaso-ganglia, which, as our investigations prove, are invariably absent in all Siluridæ.

Very little is known about the physiology of hearing in Fishes, but we are unable to see that there is any need to assume that the conditions of subaqueous audition are very different to those in air, except in so far as the physical differences in the conductivity of the respective media are concerned. Sound vibrations travel much more rapidly in water than in air and to far greater distances, but they pass with difficulty from water to air, and conversely. Those sound vibrations which are too feeble to produce any appreciable effect on the external surface of the skull when they pass through air can, nevertheless, strongly impress the ear when propagated in water and the head of the observer is completely submerged (Colladon and Sturm). Sound waves impinging on the surface of a Fish's skull would therefore be readily conveyed to the perilymph and endolymph of the ear, and such sounds will, in all probability, be heard with greater rapidity and from greater distances than could possibly be the case under similar conditions in air.

The strongest objections to the auditory function of the air-bladder and Weberian mechanism (Weber's theory) are to be found in the imperfections of the apparatus. If vibrations can pass at all from the external medium to the gases contained within the air-bladder the transmission must be accompanied by a considerable loss of intensity, and this must especially be the case in those Ostariophyseæ in which the air-bladder is widely separated from the superficial skin by the liver and other organs and tissues. In many Siluridæ the walls of the air-bladder are too thick to admit of their vibrating synchronously with rapidly recurring sound waves. The inertia of the ossicles themselves, and the interposition between them of a compressible ligament,

are insuperable objections to this theory; and, moreover, the Fish could have no appreciation of the direction of the sounds conveyed through this mechanism. Finally, it may be affirmed that, contrary to what might be expected if so complicated a structure as the Weberian mechanism is an accessory to audition, there is absolutely no evidence of the existence of exceptional powers of hearing in the Siluridæ or any other Ostariophyseæ. For these reasons we conclude that the Weberian ossicles are in no way related to the function of hearing, even to the subordinate and qualified extent tacitly suggested by Hasse and Ramsay Wright.

The only remaining view is that the ossicles under consideration are accessory to the hydrostatic function of the air-bladder. Moreau, and later, Charbonnel-Salle, have completely refuted the older theory of this function, which has usually been associated with the name of Borelli. Summarising the conclusions which the experimental researches of these authors and certain other facts appear to warrant, it may be affirmed for Fishes in general :—

(*a.*) The function of the air-bladder is to render the Fish, bulk for bulk, of the same weight as the medium in which it lives. In this mean condition, or plane of least effort, the Fish acquires a capacity for the maximum amount of locomotion with a minimum of muscular effort.

(*b.*) In its movements of ascent or descent the Fish becomes exposed to augmented or diminished pressure, which, in each case, varies in amount according to the variable height of the superimposed column of water, and this leads to an expansion or contraction of the air in the air-bladder, and consequently to an increase or diminution in the volume of the Fish itself, and thereby to a corresponding alteration in its specific gravity, which may temporarily remove the animal from its normal plane of least effort.

(*c.*) The Fish has no power of varying the capacity of its air-bladder by direct muscular contraction, and its readjustment to a new plane of least effort results from a gradual increase or decrease in the amount and volume of the air contained within the air-bladder to an extent proportional to the new pressure and due to a corresponding modification of the processes concerned in the secretion or absorption of the contained gases. Hence, by this apparently automatic method of adjustment, the Fish will find, sooner or later, and whatever may be the depth of the water and the amount of external hydrostatic or atmospheric pressure, a plane of least effort, where it will again possess exactly the density of the water.

(*d.*) That Johannes Müller's theory of the displacement of the centre of gravity upon a longitudinal axis in the case of Fishes with a two-chambered air-bladder has no foundation in fact.

(*e.*) That, despite the obvious advantages which an air-bladder con-

fers upon its possessor, there are certain attendant disadvantages, of which, perhaps, the most important is the restriction of freedom of locomotion in a vertical direction, the result of the slowness with which the necessary secretion or absorption of gas takes place.

The conclusions embodied in the preceding sections relate more particularly to the Physoclisti, by far the largest group of Teleostean Fishes, but it may be pointed out that in a general way they apply also to the Physostomi, with, however, the qualification that in the great majority of the latter group the mechanical liberation of gas through the ductus pneumaticus takes the place of absorption as a means of adjustment to reduced hydrostatic pressures.

From the conclusions established by Moreau and Charbonnel-Salle it is obvious that the varying pressures to which the gases contained in the air-bladder are exposed constitute an important factor in the physiology of locomotion in Fishes, and hence, in the absence of any other tenable hypothesis as to its function, there is a strong antecedent probability in favour of the view which Hasse was the first to suggest, viz.:—that the object of the Weberian mechanism is to bring directly to the consciousness of the Fish the varying tensions of the gaseous contents of the air-bladder, due to the incidence of varying hydrostatic pressures. The late Dr. Sagemehl also adopted Hasse's view, at least, so far as to regard the mechanism as a register of pressure variations, but with this important modification, that it is not hydrostatic but atmospheric pressure which the Fish is thereby enabled to appreciate. There are, however, certain grave objections to Sagemehl's ingenious theory.

To a Fish at a depth of, say, six feet below the surface of the water, a variation of atmospheric pressure sufficient to raise or depress a column of mercury in a barometer to the extent of half an inch will only involve a variation of pressure amounting to less than one-tenth of the already existing hydrostatic pressure; and even this trifling difference will become relatively smaller as the depth at which the Fish lives becomes greater, while the ascent or descent of the Fish in the water to the extent of only seven inches would certainly mask any variation of atmospheric pressure to the extent indicated, seeing that the animal can have no power of differentiating the effects due to the incidence of the two pressures. A barometrical variation of even half an inch takes place but slowly, and rarely occurs in less time than several hours, and consequently could only be appreciated as distinct from hydrostatic pressure if the Fish remained at exactly the same depth in the water during the whole time that the barometrical variation was in progress. The maximum range of variation in atmospheric pressure, as measured by the barometer, is about two inches, but such variations occur only at considerable intervals of time, and then may take hours to accomplish. Even in this extreme case the

atmospheric pressure variation might be negatived by a variation of level in the water to extent of twenty-seven inches, or more or less completely masked by similar movements of still less extent during ordinary locomotion, or by the rise or fall of the tide in the case of the few estuarine or marine species. It may also be urged in opposition to the theory that there is no evidence that the Siluridæ, or any other Ostariophyseæ, are in any way different from other Fishes in being specially susceptible to atmospheric pressure variations, or that they possess any special capacity for anticipating impending changes in the weather. Sagemehl's theory being untenable, Hasse's view only remains. The general structure of the air-bladder, the mode of interconnection of the different Weberian ossicles, and their relations to the air-bladder and auditory organ, as well as the relations *inter se* of the two last-mentioned structures, are perfectly consistent with this theory, against which no anatomical objections can be urged, and equally inconsistent with any other at present suggested.

III. That the Weberian mechanism is of great functional importance to the Fish possessing it admits of no doubt. It is extremely improbable that so complicated and highly specialised a mechanism would have been evolved did it not confer some exceptional advantage upon its possessor, and that this is the case seems to be clearly demonstrated by the significant fact that the presence of the mechanism is characteristic of nearly all the dominant families of fresh-water Teleostei. The precise utility of the mechanism is, nevertheless, a very difficult problem. Assuming the correctness of Hasse's theory, it is inconceivable that the pressure variations, which it is the function of the Weberian apparatus to register, can arise from any other cause than the ascent or descent of the Fish in the water during ordinary locomotion, and this at once suggests that the advantage of the mechanism is directly related to some form of pressure adjustment. In dealing with this aspect of the question it becomes necessary to first consider the methods of pressure adjustment in Fishes in general.

Gaseous secretion and absorption are highly important factors in the adjustment of the volume of the gases contained in the air-bladder to variations of hydrostatic pressure. The conditions under which these processes take place have been experimentally investigated by Moreau, who has demonstrated that when the air is exhausted from the air-bladder (by means of an air-pump in the case of Physostomous Fishes, or by puncture in the Physoclisti) it takes from several hours to several days to restore the abstracted air by secretion and for the Fish to regain its normal liberty of movement. The rate of absorption is in fairly close agreement with that of secretion. It is obvious that the rapidity with which these processes take place is an important factor in determining how far they are likely to be available as

a means of pressure adjustment during ordinary locomotion, but unfortunately, in Moreau's experiments, the amount of gas previously abstracted from the air-bladder, or the extent of the pressure variation to which the organ was exposed, was often so considerable that from several hours to several days were required to restore the normal equilibrium of the Fish. No attempt has yet been made to obtain accurate measurements of the precise rate of secretion or absorption under conditions involving relatively small variations of level and pressure. Three important factors appear, however, to be well established :—(1) that gaseous secretion and absorption are relatively slow processes in all Fishes; (2) that, although retia mirabilia are not indispensable to these processes, there can be no doubt that both take place much more rapidly in Fishes that possess such structures than in those in which they are wanting; and (3) that increased hydrostatic pressure accelerates the rate of secretion, while diminished pressure exerts a similar influence on absorption.

In the case of the Physoclisti, which very generally possess retia mirabilia, but no pneumatic duct, gaseous secretion and absorption must be the only means of pressure adjustment; but how far these methods can be employed during the more or less rapid changes of level that occur in ordinary locomotion is doubtful, and, bearing in mind the relatively slow rate at which these processes take place, even in Fishes with retia mirabilia, we incline to the opinion that they are more likely to be of advantage to the Fish during such gradual changes of level as may occur in the course of diurnal, seasonal, or other periodic migrations than in ordinary locomotion. That this is the case seems to be suggested by many features in their habits, which tend to prove that most Physoclisti have but a comparatively restricted vertical range in so far as normal locomotion is concerned.

The relatively few Physostomi which possess an air-bladder and no ductus pneumaticus are in precisely the same position, as regards their mode of pressure adjustment, as the majority of the Physoclisti; and of the remainder, we may consider, in the first place, the Ostariophyseæ, which possess not only an open pneumatic duct and a Weberian mechanism but are without retia mirabilia.

The absence of retia mirabilia in all hitherto investigated Ostariophyseæ suggests that as a means of pressure adjustment gaseous secretion and absorption are of minor importance. On the other hand, Moreau's experiments prove that those Ostariophyseæ with which he experimented possess a great advantage over the Physoclisti in that they can, during ascent, more rapidly adjust the volume of gas in the air-bladder to decreased hydrostatic pressure by mechanically liberating a certain quantity of gas through the pneumatic duct than by relying solely on the absorptive capacity of the

walls of the air-bladder. Conversely, the typical Physoclisti have the advantage during descent. Moreau points out that a Fish incurs more danger by rising above the plane of least effort than by sinking below it. It is conceivable that a Physoclist, in the course of rapid ascent, might so far depart from its normal plane of equilibrium as to be forcibly carried to the surface of the water, and in that helpless condition fall an easy and conspicuous prey to predaceous Birds or Fishes. No special danger or inconvenience would result from a sudden and rapid descent, and it is at least possible that the increased secretion of gas which augmented pressure certainly conditions in the Physoclisti may not be altogether without effect in bringing about a more speedy re-adjustment to the greater pressure of a deeper level, even in the Ostariophyseæ, in spite of the absence of retia mirabilia. From these considerations it follows that, as compared with the Physoclisti, the Ostariophyseæ possess a far greater capacity for adapting themselves to rapid and extensive changes of level, more particularly in the direction of ascent, and many well-known facts in connexion with their habits support this conclusion.

The physiological relation of the Weberian ossicles to the hydrostatic function of the air-bladder is a problem which can only be satisfactorily solved by experimental inquiry. The evidence seems conclusive against assigning more than a very subordinate part to this mechanism, if any, in the way of controlling the absorption or secretion of gas; but it may, nevertheless, control or regulate the escape of gas through the ductus pneumaticus. A gradual distension of the air-bladder would be accurately measured by the recording lever (Weberian ossicles) and the increasing intensity of the stimulus imparted to the sensory epithelium of the auditory organ and to the saccular branches of the auditory nerve. The consequent reflex or voluntary efferent impulses may find expression in the exercise of some form of regulatory control over the liberation of gas through the pneumatic duct, so that only so much gas will be eliminated from time to time as may suffice to enable the Fish to retain its plane of equilibrium at all levels during ascent, notwithstanding the reduction of external hydrostatic pressure. Unfortunately there is but little anatomical and absolutely no experimental evidence as to how, or in what way, the escape of gas is regulated in accordance with these suggestions. Valvular structures have been found in the ductus pneumaticus in some Ostariophyseæ (*e.g.*, Cyprinidæ), and we have found unstriped muscle cells in its walls. It is probable, as suggested by Ramsay Wright, that the duct is not to be regarded as a mere channel for the escape of gas from the air-bladder, but rather as a structure which, under reflex control, actively participates in the process, possibly by peristaltic contractions. The air-bladder and pneumatic

duct exhibit some structural analogies to the gall-bladder and cystic duct of Mammalia, and bearing in mind that the absence of intrinsic muscles in the walls of the bladder is associated with the fact that the tension of the contained gases under the influence of reduced hydrostatic pressure will supply the needful expulsive force, it is by no means improbable that a close physiological parallelism may also exist with regard to the escape of their respective contents.

Whatever may be the precise nature of the controlling mechanism, the advantage to the Fish of some method of carefully graduated adjustment to pressure variations is sufficiently obvious. Without any form of regulatory control, and with an open ductus pneumaticus in free communication with the exterior, it may be surmised that the escape of gas would be continuous and unchecked, and might even involve a more or less complete exhaustion of the gas in the bladder as the pressure diminished, with the contingent disadvantage that the normal equilibrium of the Fish in the water would be greatly disturbed, and a considerable demand be made on the secretive activity of the bladder for the subsequent restoration of the gas. On the other hand, the existence of a controlling mechanism would ensure that only so much gas will be evolved as may suffice to maintain the Fish in a plane of equilibrium, and, at the same time, secure the needful economy in the liberation of the gas. A further advantage in the speedy adjustment to alterations in pressure is that there will be less expenditure of energy in the performance of ordinary movements, inasmuch as the Fish can readily find a plane of least effort; otherwise it would have to counteract a too feeble, or an increased specific gravity, by additional muscular effort. In the light of these considerations, but with the qualification which the absence of direct experimental evidence necessitates, we conclude that the Weberian mechanism not only confers on all Fishes that possess it an exceptional capacity for freedom of locomotion in the vertical direction, but also entails the contingent advantage that all movements will be effected with the maximum economy of muscular effort and tissue metabolism.

With regard to those Physostomi which have no Weberian mechanism, the evidence as to their ability to make use of the ductus pneumaticus as a means of pressure adjustment is very conflicting, and, moreover, it is not even certain that in all cases the duct is in free communication with the exterior. The frequent presence of retia mirabilia would also suggest that gaseous secretion and absorption are important factors in pressure adjustment. In the light of such contradictory evidence, no satisfactory conclusion is, at present, possible, but two alternative suggestions may be made. (*a.*) If the ductus pneumaticus cannot, from any cause, be used for pressure adjustment, gaseous secretion and absorption must be the only methods employed,

and, so far as the point is concerned, these Fishes must resemble the typical Physoclisti. (*b.*) On the other hand, even if it be admitted that some Physostomi, without the Weberian mechanism, can liberate gas through the ductus pneumaticus, it is nevertheless not difficult to see how it may be that the process is of little use to them for pressure adjustment. The completeness of the control exercised over the liberation of the necessary amount of gas will largely depend on the perfection of the reflex mechanism employed in the process, and in all the Fishes now under consideration the necessary afferent impulses must be initiated in the peripheral nervous system by the diffused pressure exerted by a distended air-bladder on the surrounding organs, instead of in a particular afferent nerve by a stimulus applied to a localised sensory epithelium through the Weberian mechanism, as is the case with the Ostariophyseæ. The indefinite character of the stimulus in the former would certainly militate against any delicacy in the responsive process of pressure adjustment. The more perfect afferent mechanism of the Ostariophyseæ conditions a more effective control over the function of the pneumatic duct, and a greater capacity for regulating the processes involved in pressure adjustment, and, as we have suggested, this is the great advantage which the Weberian mechanism confers upon all Fishes that possess it.

Certain structural adjuncts in connexion with the air-bladder of the Siluridæ may also be considered from a physiological point of view. These are, (1) the lateral cutaneous areas; (2) the "elastic-spring" apparatus of Müller; (3) the extrinsic muscles of the Pimelodinæ; and (4), the distinctive features of the air-bladder and Weberian mechanism in the Siluridæ as compared with other Ostariophyseæ.

1. The lateral cutaneous areas probably enable variations in the size of the anterior chamber of the air-bladder, the result of pressure variations, more promptly to modify the volume and therefore the specific gravity of the Fish, and consequently ensure a corresponding increase in the delicacy of the responsive processes involved in pressure adjustment.

2. Müller held the opinion that the "elastic-spring" apparatus is a mechanism for the condensation and rarefaction of the gases in the air-bladder. We cannot agree with Müller that the elastic springs can have any share in dilating the air-bladder or rarefying the gases which it contains, and it is doubtful if the apparatus can possibly give the Fish any power of directly compressing the bladder except under certain conditions, viz., when the anterior chamber becomes distended through the diminution of pressure which occurs during ascent in the water, coincidently with the forward or outward movements of the two springs as the result of the voluntary reflex con-

traction of their protractor muscles. We do not think with Müller that condensation is of use in facilitating descent; for a Fish in approximate equilibrium the slightest action of the ordinary locomotor organs is quite sufficient to produce either ascent or descent, and the existence of an elaborate mechanism for varying the internal capacity of the air-bladder and the volume of the enclosed gases with this object is altogether unnecessary. Sorensen's view that the mechanism is related to the production of voluntary sounds by the forcible expulsion of air through the pneumatic duct does not seem to us well founded, first, because the "elastic springs" are only able to forcibly compress the air-bladder during ascent, and, secondly, because sounds could only be produced in that way at the expense of a considerable disturbance of the normal equilibrium of the Fish in the water and of its locomotor activity. Two views may be taken as to the precise mode in which this singular mechanism is of practical utility: (1) the compression of the air-bladder may assist the action of the ductus pneumaticus in producing a more rapid ejection of gas during ascent; or (2) by condensation alone may counteract the effects of a too low specific gravity, and, at the same time, economise the contained gases. The latter alternative, in our opinion, is the more probable one.

3. The extrinsic muscles (compressor muscles) of the Pimelodinæ in all probability have a function similar to that of the "elastic-spring" apparatus. The function of the tensor tripodis muscle is probably to limit the violent excursions of the tripus, which otherwise would certainly take place when the bladder is forcibly compressed by the contraction of the compressor muscles.

4. The conclusion suggested by a comparison of the anatomical relations of the air-bladder and its associated skeletal structures is—that, physiologically considered, the most important distinctive features of the Weberian mechanism in the Siluridæ as compared with other Ostariophyseæ are mainly related to the air-bladder, which in the former attains its maximum degree of specialisation and delicacy as an organ adapted for the registration of varying hydrostatic pressures.

The Condition of the Air-bladder in the Siluridæ Abnormales.

The most noteworthy features in the structure of the air-bladder in the various genera of Siluridæ abnormales are: the absence of lateral chambers; the partial or complete constriction of the anterior chamber into two diminutive laterally situated air-sacs, which may lose all connexion not only with each other, but also with the œsophagus; and the occasional atrophy of the fibres by which the lateral walls of the bladder are normally connected with the tripodes.

Indications of retrogressive changes are not wanting in the auditory organ. In some, at least, of the Siluridæ abnormales the sinus endolymphaticus has completely atrophied, although the cavum sinus imparis and atrial cavities remain and retain their normal relations to one another and to the scaphia. The Weberian ossicles, on the other hand, are almost invariably complete. The few signs of degeneration which they exhibit relate to the straightness of the posterior processes of the tripodes, the suppression of the ascending and condylar processes of the scaphia, and, in a few instances, the absence of intercalaria.

Taking into consideration the retrograde changes both in the air-bladder and Weberian mechanism, it becomes almost impossible to believe that any hydrostatic function can be assigned to these structures, or that they do otherwise than present various states of modification towards the condition of vestigial and functionless organs, and this conclusion seems to us equally inevitable whatever may have been their original function.

Of the one hundred and sixteen genera mentioned in the British Museum Catalogue no fewer than twenty-five at least are referable to the Siluridæ abnormales. The causes that have led to the degeneracy of the air-bladder in so many forms are in many instances not difficult to trace, and, as in so many Physoclist Teleostei, the assumption of a purely ground habit of life is probably the most important one. Not a few of the genera of Siluridæ abnormales inhabit the comparatively shallow waters of rapidly flowing mountain streams and torrents, often being at a considerable altitude, and in general habit are not unlike our common English Loaches. Many are provided with an adhesive apparatus on the ventral surface of the body between the pectoral fins, so that they may be enabled to withstand the force of mountain torrents. Such Fishes when not in motion probably rest upon or attach themselves to the river bottom, and the uselessness and probable harmfulness of an air-bladder as a hydrostatic organ under such conditions is no doubt the cause of its degenerate and rudimentary condition. Various species of *Callichthys* are said to keep under plants in marshy swamps, to be able to burrow in the mud, in which they often become dried up, and even to be capable of migration upon land in search of water; similar habits characterise other forms.

The susceptibility of the air-bladder to change of habitat or habits on the part of its possessor is well shown by the variation that may be met with in the same genus. Two species of *Cryptopterus* (*C. micropus* and *C. hexaptera*) have rudimentary air-bladders, while all the remaining species of the genus which came under our notice have these organs unusually well developed. In two species of *Pimelodus* also (*P. pulcher* and *P. sapo*) the air-bladder is not only

rudimentary but lacks even a trace of the compressor and tensor tripodis muscles which are so characteristic of the normal Pimelodinæ. Probably in such instances the degeneration of the air-bladder is due to the assumption of a ground habit.

The invariable persistence of the Weberian ossicles in an almost structurally complete but functionless condition may be explained by the absence of any potent cause calculated to bring about their total suppression.

The uniform retention of the anterior chamber in these Siluroids while the lateral compartments have almost invariably disappeared, in place of the entire suppression of the air-bladder which occurs in most other Teleostei whenever that organ from similar causes has become useless to its possessor, is due to its connection with the persistent Weberian ossicles.

The encapsulation of the air-bladder by bone is difficult to explain satisfactorily, but two alternative suggestions may be made. (1.) Encapsulation varies greatly in extent as well as in the precise methods by which it has been brought about, but very often appears to bear some relation to the structural completeness of the diminutive air-bladder and the retention of its apparently functional connexion with the Weberian ossicles. Where the air-bladder retains much of its structural integrity encapsulation is always more complete than when the contrary is the case, and may then be due to the necessity of preventing any distension of the reduced and useless organ by varying external pressures from imparting disturbing and useless stimuli to the internal ear. On the other hand, encapsulation is always less complete when structural lesions are too obvious to admit of any possibility of pressure variations affecting the Weberian ossicles, and the fact that it exists at all in such cases may be explained by the supposition that reduction in the size of the only portion of the air-bladder that persists, that is, the anterior chamber, has been accompanied by a corresponding contraction and curvature of the modified transverse processes which normally invest, and are closely moulded to, its anterior and dorsal surfaces. (2.) Encapsulation may be due in part to the tendency of the transverse processes to contract round and envelope an atrophying air-bladder, and in part also to an unchecked development of that tendency to ossification of the walls of the air-bladder, or of the investing connective tissue, which to a restricted extent is so characteristic a feature even in the normal Siluroids.

Of the remaining families of the Ostariophyseæ, the Cyprinidæ exhibit a substantially similar and parallel series of modifications in the condition of the air-bladder, which, it can scarcely be doubted, are also correlated with the assumption of a grovelling and purely "ground" habit of life, and, as in certain Siluridæ abnormales, the

invariable completeness of the encapsulation of the reduced air-bladder by bone in the former may be associated with the fact that both the bladder and the Weberian mechanism otherwise retain their normal structural integrity.

Geographical Distribution of the Ostariophyseæ.

All the Ostariophyseæ are fresh-water forms, except a few Siluroids which have become accustomed to a marine habitat. Of the known 2180 (approximate) fresh-water Teleostei, there are only about 600 in which the Weberian apparatus is absent. Broadly speaking, it may be stated that the total number of species of Ostariophyseæ is nearly five times as great as all the other species of fresh-water Physostomi; nearly five times greater than the species of fresh-water Physoclisti; and about three times the number of the species of Physostomi and Physoclisti combined. With the exception of the Gymnotidæ and Gymnarchidæ, the families of the Ostariophyseæ are, so far as species are concerned, from two to six times as large as the best represented families of the remaining Physostomi or the Physoclisti; and the families of the Cyprinidæ and Siluridæ are by far the richest, both in species and genera, the former including one-third (724), and the latter about one-fourth (572), of all the known fresh-water species. This predominance characterises all the great zoogeographical regions about which we have any information, with the exception of North America, where other fresh-water Fishes slightly outnumber the Ostariophyseæ. In the Indian and Neotropical regions, where fresh-water Fishes attain their maximum degree of specific development, the Ostariophyseæ outnumber all the remaining species in those districts in the proportions of 5·5 and 4 to 1 respectively. The great rivals to the Ostariophyseæ among fresh-water Fishes are the Salmonidæ and Cyprinodontidæ, but, owing to a difference of habit, or of geographical distribution, a considerable number of their species do not come into direct competition with the former.

Hence, it may be concluded that the possession of a Weberian mechanism is specially characteristic of the dominant families of fresh-water Teleostei, that is, of those families which combine to the greatest extent numerical strength in individuals, richness in specific differentiation, and wideness of geographical distribution.

Two other possible conclusions are also suggested by these facts: (1) that the possession of a Weberian mechanism is directly related to certain peculiarities of a fresh-water habitat, and (2) that the close association between the presence of this mechanism and the marked ascendency of the Ostariophyseæ over all other families of fresh-water Teleostei points to the possibility that the relation is one of cause and effect. Both conclusions, in our opinion, are highly probable, and the

demonstration of the first would almost necessarily involve the truth of the second, but positive proof of either is extremely difficult. To prove the correctness of the first, it must be shown that there are certain conditions involved in a fresh-water habitat which render the possession of a Weberian mechanism of special value to fresh-water Fishes. If, as we have suggested, the mechanism confers upon all Fishes which possess it an exceptional capacity for locomotion in all directions, with a minimum of muscular effort, it seems reasonable to anticipate that such advantages are of greater importance to fresh-water species than to their marine congeners.

The poverty of fresh-water faunas, as compared with marine, and the entire absence of certain groups of organisms which are abundant in the sea, are among the most obvious facts in the geographical distribution of animals. Of the special external conditions which have combined to produce these results, the most obvious, and perhaps the most important, are, (*a*) existence in a medium which, omitting lakes, is always in motion in a definite direction; and (*b*) a more precarious and fluctuating food supply, due to climatic severity, alternations of seasons (such as winter and summer, dry and rainy seasons), and to the isolation and comparative smallness of fresh-water areas. On the other hand, the relation of Fishes to other forms of fresh-water life is in many respects unique. No other groups of equivalent taxonomic value approach Fishes in richness of specific differentiation or individual size, and it is equally clear that the predominancy of Fishes is mainly due to the numerical strength of the Ostariophyseæ, both in individuals and in genera and species. It is, therefore, not unreasonable to infer that, for some reason, the special conditions of a fresh-water existence are less adverse to Fishes than to any group of organisms, and, further, that the Ostariophyseæ apparently possess an altogether exceptional capacity for adapting themselves to conditions which, in almost all other cases, are inimical, both to individual and specific development. Can it be shown that there is any definite relation between any of the conditions of a fresh-water life and the advantages derivable from the possession of a Weberian mechanism?

Of the special conditions of a fresh-water habitat, we lay most stress on the precarious and fluctuating character of the food supply, and a comparison of the relative positions of fresh-water and marine Fishes from this point of view suggests a possible answer to the question. The food supply of marine Fishes is less precarious and less liable to quantitative fluctuations than in the case of fresh-water species, and, moreover, local scarcity of food may be met by migration to areas where, for the time being, food is more abundant. With fresh-water Fishes the converse holds good, and in their case the restricted extent and comparative isolation of fresh-water areas are,

in general, an insuperable obstacle to migration as a means of remedying local scarcity. The poverty of most other forms of fresh-water life absolutely conditions the existence of a relatively larger number of herbivorous or omnivorous Fishes in fresh waters than in the sea, where the abundance and variety of other animal organisms are so much greater, and this necessarily involves the existence of a piscine fauna, which, from the nature of the food supply, is to a large extent peculiarly liable to the exigencies of a precarious and inconstant food supply. It can also be shown that marine Fishes are more voracious than fresh-water, and that while the latter may survive total abstinence from food for weeks or months, the former succumb within a few days. The majority of the Ostariophyseæ appear to be herbivorous or omnivorous, while the capacity of many of them for accumulating reserve food material, at the expense of which they live during the seasons of relative scarcity, has often been remarked.

Not only do marine Fishes differ from the majority of fresh-water species in the greater constancy and abundance of their food supply, but they also differ from the latter in their method of pressure adjustment. Relying, as the former do, upon the relatively slow processes of gaseous secretion and absorption, any departure from the plane in which, for the time being, they are in equilibrium must involve a decrease or increase of specific gravity to an extent proportional to the amount of pressure variation. Hence, in ordinary rapid locomotor movements, involving more or less extensive changes of level and pressure, there must be an increase of muscular exertion, which will necessarily be greater in proportion as the Fish departs from its normal plane of equilibrium. In the great majority of fresh-water Fishes, that is to say, in the Ostariophyseæ, pressure adjustment is more accurate and rapid, so that in all variations of level and pressure, whether rapidly or slowly brought about, but more especially under conditions of diminished pressure, the Fish always retains a normal plane of least effort, with the result that its locomotor movements will be accompanied by a minimum expenditure of muscular effort. It may therefore be inferred that, as a general rule, marine Fishes are exposed to greater demands upon their muscular energy than is the case with fresh-water species—a difference which must always be associated with the more favourable nutritive conditions under which the former exist, as compared with the more precarious food supply of the latter—while, at the same time, it affords a reasonable explanation of the relative capacities of marine and fresh-water Fishes for enduring prolonged abstinence from food.

If we do not overrate the importance of these considerations, it is obvious that, in view of the precarious and fluctuating character of

their food supply, economy in the expenditure of muscular energy must be of primary importance to the majority of fresh-water Fishes, and more particularly to those which, wholly or in part, derive their food from vegetable sources. Hence, the possession of any mechanism which will not only remove all restrictions to motion in the vertical direction, and thereby enlarge the area within which food may be obtained, but, at the same time, will also enable those Fishes to execute all locomotor movements with the least possible expenditure of muscular effort, must prove to be a great physiological advantage to them, inasmuch as economy of muscular effort implies diminished tissue metabolism, and, consequently, indirectly but effectually aids the accumulation of the nutritive reserve, at the expense of which the Fish subsists during the lengthened periods when fresh food is relatively scarce. It may be that this is precisely the advantage which the possession of a Weberian mechanism enables all Ostariophyseæ to realise—an advantage which, as we venture to suggest, is one of the main causes of their marked ascendency over all other fresh-water species in which this mechanism is wanting, and with which they come into direct competition.

As no other attempt has been made to associate the evolution of the Weberian mechanism with any special peculiarities of external environment, we would suggest the following tentative conclusions:—

1. The special feature of a fresh-water habitat that has conditioned the development of the Weberian mechanism in the Ostariophyseæ is the occurrence of seasonal or periodic quantitative variations in the food supply, variations to which the Ostariophyseæ, from their herbivorous or omnivorous habits, are specially liable.

2. In view of such unfavourable nutritive conditions, the special advantage which is conferred upon the Ostariophyseæ by the possession of the Weberian mechanism is a capacity for executing locomotor movements in any plane, with an almost irreducible minimum of muscular effort and tissue metabolism.

3. If a variable and inconstant food supply is to be regarded as one of the inevitable conditions of a fresh-water existence, and necessitates strict economy in the expenditure of muscular energy, any mechanism which secures this result must be of unquestionable importance to the species, and hence it may be that the Ostariophyseæ owe their dominant position among fresh-water Fishes to the possession of the Weberian mechanism.

4. The evolution of the Weberian mechanism has not only conditioned the predominancy of the Ostariophyseæ, but, indirectly, has favoured the existence in fresh water of a large number of purely carnivorous Fishes, which depend on the former for their food, and therefore may also be regarded as one of the primary causes of the

anomalous abundance and diversity of fresh-water piscine life, as compared with the remarkable poverty of all other groups of fresh-water organisms.

Concluding Remarks.

The varied structural modifications met with in the air-bladder of the Siluridæ are not surprising, in view of the exceptionally diversified conditions under which the different species and genera live. The physical conditions under which many Siluridæ are capable of living are almost as varied as their geographical distribution or climatic range.

Darwin has pointed out, in referring to the conditions favourable to variation in animals and plants, that it is common, widely diffused, and widely ranging species that vary most, and that this might be expected from the diverse physical conditions to which they would be exposed, as well as from differences in the nature and quality of their living competitors in different regions. Further, he alludes to the fact that an organ developed in an extraordinary manner implies that it is of high functional importance to the species, and that it may also be concluded that the organ has undergone a great amount of variation since it first came into existence. It is clear that the Siluridæ furnish an admirable illustration of the truth of these remarks.

Nor is it difficult to see how it is that the Weberian apparatus and air-bladder are more specialised in the Siluridæ than in other Ostariophyseæ. The only rivals to the Siluridæ in the extent of their geographical distribution are the Cyprinidæ, for the remaining families have but a comparatively restricted range. But, extensive as is their geographical distribution, the great majority of the Cyprinidæ appear to exist under fairly uniform conditions, or, at all events, exhibit nothing like the diversity of habitat and habits that is so characteristic of the Siluridæ, and hence it is that, so far as the structure of the air-bladder and Weberian mechanism is known in the former family, it presents but little variation in the direction of increased specialisation, although in a few genera the effects of degeneration are sufficiently obvious.

XIV. "On the early Development of Cirripedia." By THEO. T. GROOM, B.A., B.Sc., F.G.S., Demonstrator in Zoology at the Yorkshire College, Leeds, late Scholar of St. John's College, Cambridge. Communicated by ADAM SEDGWICK, F.R.S. Received May 11, 1892.

(Abstract.)

During a month's occupation of a table at the Marine Biological Laboratory of Plymouth, in the summer of 1889, and a nine months' occupation of the Cambridge University Table at the Zoological Station of Naples, commencing in the October of the same year, I had the opportunity of studying the development of a number of Cirripedes. Since that time, investigations on the same group have been carried on chiefly in the Morphological Laboratory at Cambridge.

I have been wishful to take up this subject, because the embryology of Cirripedia had been considerably neglected since the introduction of the more modern methods of investigation, and because the development might reasonably be expected to throw light on the adult structure of this interesting group. I wished, moreover, to compare the development in several species, with the object of throwing some light on larval evolution in general.

The species studied were *Lepas anatifera*, Linnæus; *Lepas pectinata*, Spengler; *Conchoderma virgata*, Spengler; *Dichelaspis Darwinii*, Filippi; *Chthamalus stellatus*, Poli, and *Balanus perforatus*, Bruguière.

Of *Dichelaspis*, only mounted specimens of the nauplii were examined, but in the other forms the development from the freshly-laid ovum to the second nauplius stage was investigated.

I had expected to find notable differences in the development of the different forms, but, although most of the genera could be distinguished at an early age, by some feature or other, the general course of development was very uniform, and the following summary is applicable to all the members of the group:—

The freshly-laid ovum consists of granular protoplasm, hollow yolk granules, and oil globules. Its size has much more relation to that of the nauplius than to that of the adult.

First polar body is given off, not in the ovary, but in the mantle cavity, though the first directive spindle is evidently formed in the ovary. The polar body is formed independently of, and probably before, or simultaneously with, fertilisation.

Fertilisation takes place in the mantle cavity before the peri-vitelline membrane is formed.

The emission of the first polar body is immediately followed by the formation of the vitelline (or peri-vitelline) membrane, which arises

while the egg is in the mantle cavity, whether the egg has been fertilised or not.

If fertilisation has not taken place, no further change ensues, and the egg does not contract; a second polar body is probably formed, but owing to the resistance of the peri-vitelline membrane cannot emerge, and is not seen.

If fertilisation has taken place, the egg diminishes in size, and commences to undergo rhythmical contractions, which cease only when the protoplasmic and yolk portions are completely separated.

The diminution in size is soon followed by the protrusion of clear amœboid processes at the anterior end of the egg, which are as often withdrawn: from this amœboid arises the second polar body, like the first, by the division of the nucleus in the ordinary karyokinetic manner.

The protoplasm generally collects at the anterior (larger) pole, and the yolk at the posterior (pointed) pole, in the well-known way. The process does not represent a total division, as has been supposed, into ectoderm and endoderm, but the formation of a teleolecithal egg, the protoplasmic part of which will form the first blastomere, and now rests upon a yolk portion, at first devoid of a special nucleus, but still in communication with the protoplasmic half.

The nucleus, at first small and peripherally situated at the anterior pole (invisible without special preparation), becomes visible as a clear spot or vesicle—the segmentation nucleus—occupying the centre of the protoplasmic half of the egg.

The nucleus divides, one daughter-nucleus remaining in the protoplasm, and the other passing into the yolk, the elements of which it has the power of transforming into protoplasm; this, together with the bulk of any protoplasm left in the yolk, now emerges as a second blastomere at the side of the first, which has in the meantime become cut off from the yolk.

The yolk becomes gradually covered by the successive emergence of fresh cells, which process is accompanied by the division of the cells cut off from it. The nucleus of these emerging protoplasmic bodies or *merocytes* is given off either from a peripheral blastomere, which has not yet been cut off from the yolk, or from a merocyte which divides before emerging as a blastomere. The yolk may be regarded as having the value of a single cell (macromere), which gives off a succession of blastomeres (micromeres) much in the same way as in the case of the epibolic eggs of *Bonellia*, where, however, there are four macromeres, each of which behaves in the same way.

The point where the blastoderm last covers the yolk represents (except possibly in rare cases) the blastopore, the nucleus which gives rise to both endoderm and mesoderm arising at or close to the same spot.

After separation of the epiblast the yolk cell or macromere remains still as a cell with a single nucléus, derived from that of the merocyte which formed the last or one of the last blastomeres. This yolk cell represents both mesoblast and hypoblast.

The meso-hypoblast cell immediately divides into two cells, one situated more dorsally, the other more ventrally. Each of these contains mesoblastic and hypoblastic elements. The mesoblast is formed by the cutting off in succession of segments from each of the two meso-hypoblast cells; these form a plug of rapidly dividing cells just in front of the closed blastopore. When all the mesoblastic cells are cut off the two yolk cells left remain as the first two hypoblast cells.

The two cells thus formed become divided into smaller yolk endoderm cells equivalent to the secondary yolk pyramids of Decapods (Reichenbach's *Secundäre Dotterpyramiden*).

Each yolk pyramid becomes later converted into an endoderm cell by radial contraction in a centrifugal direction, accompanied by gradual retreat of the nucleus to the periphery; the archenteric cavity arises by the separation thus caused of the central portions of the pyramids from one another.

The alimentary canal arises in three divisions, as in the Arthropoda generally, the stomach being formed mainly from the yolk endoderm, and the lining of the œsophagus and intestine probably wholly as long epiblastic ingrowths (stomodæum and proctodæum).

The mesoblastic cells of the nauplius, arising in the way described, divide up rapidly and extend forward between the ectoderm and endoderm as a dorsal plate; this soon grows down at the sides, but does not at first extend to the ventral surface.

This plate is chiefly concerned in the formation of the muscles of the nauplius appendages, which arise, as is probably the case in all nauplius forms, with the free ends directed dorsally.

The appendages are marked out first by two transverse furrows dividing the embryo into three segments; these occur only across the dorsal surface and up the sides, not extending into the ventral surface. Very soon the dorsal surface becomes traversed by a median longitudinal furrow, which does not extend to the ends of the body, but is bounded by two new transverse furrows; these furrows mark out an anterior and posterior (caudal) unpaired lobe with the free ends of the appendages between them on the dorsal, and not, as has always been stated for Cirripedes, on the ventral surface.

The antennules, antennæ, and mandibles are probably serially homologous, as indicated by their similarity in the free nauplius, and by their remarkable and similar origin; all may represent primitively post-oral appendages.

There are no mesoblastic somites at any period of embryonic

development; but the mesoblastic layer becomes thickened locally to form the muscles of the appendages in each of the three segments.

The body cavity* of the nauplius arises later as a mixed blastocœle and schizocœle, due in part to the separation of ectoderm and endoderm, and in part to an excavation of mesoblastic tissue. It soon forms a cavity continuous from end to end of the body.

The nervous system of the nauplius (arising as usual as an epiblastic thickening) shows from the beginning a complex structure, especially in the Balanids, and among these it is most specialised in the Balaninæ. It is probably from the first a syn-cerebrum, since it includes, in addition to the representative of the archi-cerebrum, the ganglia supplying the antennules. The antennæ and mandibles are in close relation with the circum-œsophageal connectives and sub-œsophageal ganglion respectively.

A comparatively sudden change is experienced by the nauplius in passing from stage I to stage II; this necessitates a telescoping of the tail, caudal spine, and bristles of the appendages, the gradual evagination of which gives rise to the peculiar appearances seen at this stage, and which have given rise to some misconceptions.

There is a most remarkable agreement between the nauplii of the various species in the general structure of the carapace, labrum, &c., extending to the minutest detail in the case of the appendages, and indicating that the features in question have been inherited from some stage of the common ancestor.

There are, however, points of difference which concern chiefly the carapace (with its horns and caudal spine), labrum, and tail.

Differences are perceptible in most cases in the new-laid ovum of different forms, and the genera, or even the species, can thus be separated even at this early stage.

The larval differences necessitate a classification which agrees very closely with that deduced from the structure of the adults.

Such differences have, however, in most cases been acquired independently of adult structure, since they concern characters peculiar to the larvæ, and lost by the adult. Some indications of the precocious appearance of characters originally belonging to the later stage are seen in *Balanus*; but most of the characters cannot be so explained. The larva and adult have varied simultaneously, but in quite different ways, each having in this group taken its own course of evolution.

The agreement in the development of such forms as *Balanus* and *Lepas*, stage by stage, indicates that the ancestor of the Thoracica underwent a metamorphosis similar to that of the present members of the group. The *Nauplius* and *Cypris* stage have, therefore, not been evolved within the group.

* This term is used in a purely descriptive sense.

The embryonic development, though in its main lines very uniform throughout the group of the Thoracica (Lepadidæ, Verrucidæ, Balanidæ), shows considerable variation in some respects, and the variable features are the same in all the species.

The most conspicuous variations are those which affect the processes of cell division. The details of the mode of growth of the blastoderm over the yolk, from the appearance of the basal plane to the closure of the blastopore, and the resulting cell arrangements vary indefinitely. After the closure of the blastopore, the yolk endoderm cells present in their mode of division an almost equally great diversity.

The size, shape, and colour of the ova and embryos of a species vary not inconsiderably.

In size and shape the nauplii of a species vary somewhat; but no conspicuous variations occur in structure, the larvæ always showing a great amount of uniformity, even in so minute a feature as the character of each bristle belonging to an appendage. Minute variations occur in the ornamentation of the carapace, caudal spine, and tail, and (in *Chthamalus*) in the number of teeth at the end of the labrum.

XV. "Thermal Radiation in Absolute Measure." By J. T. BOTTOMLEY, M.A., D.Sc., F.R.S. Received June 16, 1892.

(Abstract.)

The paper contains an account of an experimental investigation by the author in continuation of researches on the same subject which have been already published ('Roy. Soc. Proc.,' 1884, and 'Phil. Trans.,' 1887). In the earlier experiments metallic wires heated by an electric current were used. The loss of heat from a heated body, however, depends to some extent on the form and dimensions of the body, and it seemed important to experiment on the loss of heat from bodies differing in form from the wires already used, and larger in dimensions.

Accordingly, two copper globes used by Mr. D. Macfarlane in 1872 ('Roy. Soc. Proc.,' 1872, p. 93) were employed for a new series of experiments.

After preliminary experiments (using the same enclosure which Macfarlane employed, and with the surfaces of Macfarlane's globes prepared in four different ways) new apparatus was constructed; the object being to experiment both with full air pressure and with different amounts of exhaustion of the air, and Macfarlane's enclosure being unsuitable for this purpose.

In the arrangement adopted, the heated globes were hung at the

centre of a hollow metallic sphere, which was connected with the Sprengel pump and surrounded with cold water, and were allowed to cool. The temperature of the cooling globe was read off at equal intervals of time by means of a thermo-electric junction; and from these readings the absolute loss of heat per unit of cooling surface, per unit difference of temperatures of surface and surroundings, per unit of time, is calculated.

The details of the apparatus and method of experimenting are given in the paper. It is enough to say here that the globes were used with their surfaces in two different conditions:—(1) Thinly coated with lamp-black, and (2) silvered and brightly polished; and in both conditions the absolute loss of heat, both in air and in vacuum, more or less complete, was determined. The tables and curves attached to the paper give the details of the results.

To quote one or two examples:—With the sooted surface a total loss of heat by convection and radiation of $3{\cdot}42 \times 10^{-4}$ c.g.s. units per square centimetre, per second, per 1° C. of difference of temperatures of globe and surroundings, was observed with a difference of temperatures of 100 C., and with the surroundings at about 14° C. Under similar circumstances the radiation in vacuum of $\frac{1}{2}$M (half-a-millionth of atmospheric pressure of non-collapsible gas) was about $1{\cdot}40 \times 10^{-4}$.

Taking a silvered and brightly-polished surface under the same circumstances, the loss in full air was $2{\cdot}30 \times 10^{-4}$ c.g.s.; and with the highest vacuum and brightest polish obtained, it was reduced $1{\cdot}80 \times 10^{-5}$ with in this case a difference of temperatures of 180° C. The loss with 100° C. difference would be considerably less, but is not known experimentally at present.

The author returns thanks to Mr. James H. Gray, M.A., B.Sc., for excellent assistance given; and expresses himself most deeply indebted, both for assistance in experimenting and calculating of the results, and for most valuable and ingenious aid of various kinds during the course of this work, to his friend Mr. A. Tanakadate, now Professor in Tokio, Japan.

XVI. "The Cerebrum of *Ornithorhynchus paradoxus*." By Alex. Hill, M.D. Received June 16, 1892.

(Abstract.)

The brain of *Ornithorhynchus paradoxus* is by no means Avian in type. All its characters are Mammalian, but it presents certain peculiar features which have been overlooked or misunderstood by the anatomists who have hitherto examined it with the naked eye. The most obvious and noteworthy departures from the form of brain

found in all other Mammals concern the rhinencephalon, the hippocampus, and the cerebral commissures.

The olfactory bulb is quite free from the frontal portion of the hemisphere. Its stalk is exceedingly thin, and connected with the under surface of the hemisphere near the median line. The portion of the mantle which receives it is separated from the general surface by a deep incision (ectorhinal fissure), which extends in depth almost to the mesial surface. The pyriform lobe thus formed merges with the general surface far back on the mesial aspect of the hemisphere, at the spot at which the hippocampal fold commences.

The hippocampus is very extensive; it lies entirely dorsal to the velum interpositum, and is continued forwards to the extreme anterior end of the brain. With the rhinencephalon it forms therefore a loop, open in front.

No commissural fibres cross the incisura pallii longitudinalis dorsally to the hippocampus. For reasons stated in his paper, the writer considers that fibres which cross from one hemisphere to the other on the peduncular (portal) side of the fascia dentata cannot be homologous with the corpus callosum, and he therefore concludes that this structure is completely absent from the brain of *Ornithorhynchus*. A strong commissure or decussation lies within the concavity of the rhinencephalic loop, but its fibres are restricted in their distribution to the hippocampal fold, as shown in sections stained after Weigert's method. The convex portions of the mantle are entirely dependent upon the anterior commissure for mutual connexion.

Exception being made to the *incisura rhinalis* and *dentary fossa* as not belonging to the category of fissures, the cortex is completely destitute of convolution.

Each hemisphere of the brain was cut into a series of sections, the anatomical features of which were described in detail.

XVII. "Contribution to the History of the Interchange of Pulmonary Gases in the Respiration of Man." By WILLIAM MARCET, M.D., F.R.S. Received June 9, 1892.

[Publication deferred.]

XVIII. "Magnetic Properties of Pure Iron." By FRANCIS LYDALL and ALFRED W. S. POCKLINGTON. Communicated by J. HOPKINSON, F.R.S. Received May 4, 1892.

[Publication deferred.]

XIX. "On the Alimentary Canal of *Pontia brassicæ.*" By A. B. GRIFFITHS. Communicated by Professor HUXLEY, F.R.S. Received April 1, 1892.

XX. "On a new Method for the Bacteriological Examination of Water, and on a new Bacillus discovered in Rain-water." By A. B. GRIFFITHS. Communicated by Dr. KLEIN, F.R.S. Received May 18, 1892.

The Society adjourned over the Long Vacation to Thursday, November 17.

Presents, June 16, 1892.

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"On a Multiple Induction Machine for producing High Tension Electricity, and on some remarkable Results obtained with it." By LORD ARMSTRONG, C.B., F.R.S.

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[PLATES 2—13.]

It is now nearly half a century since I undertook to design, for the Polytechnic Institution then existing in London, a large hydro-electric machine, on the plan of the smaller one I had previously designed for myself. It was a very short time in my hands after its completion, and I had scarcely any opportunity of afterwards using it in the lecture room of the Institution. My experience with it was, however, sufficient to show me that its great power was very much less when used in a room than in the outside atmosphere when dry. I have ever since entertained the idea of constructing a similar machine of equal or greater power for my private use; but, until lately, the exigencies of business pursuits precluded my giving attention to the subject.

On recommencing my experiments on this branch of electrical science, my immediate object was to improve the frictional apparatus of the steam jet, so as to obtain the greatest effect from a given expenditure of steam, while my ultimate intention was to construct a large machine with such a number of jets as would afford me a more copious supply of frictional electricity than could be obtained by any other convenient means. I soon found, however, that the difficulty of obtaining effectual insulation in the open air, except under the most favourable conditions of weather, would involve great interruption and disturbing effect, and I therefore turned my thoughts to the induction coil as a source of high tension electricity, that would not only be independent of the caprice of the weather, but would also save me from atmospheric inclemency, which, however harmless it might have been in the youthful days of my hydro-electric experiences, could not be safely endured at my present advanced period of life.

I did not flatter myself that I could make any important improvement upon the Ruhmkorff coil. That remarkable instrument had been so long in use, and had undergone so much development, that its career of progress might well be considered as nearly, if not quite, played out. But although Ruhmkorff coils had been constructed capable of yielding sparks of unprecedented length, yet it was obvious that the output of electric energy, when estimated in ampère as well as in volt measurement, must be relatively very inferior to that of smaller coils, in which each convolution of the

secondary wire is effected with much less length of wire. In other words, a given weight of material utilised in a number of small coils ought to yield a greater output of energy than the same weight used in the construction of one large coil, the best proportions of length to diameter being in each case adhered to. Moreover, it is well known that, with induction coils of exceptional magnitude, the ordinary vibrating contact breaker cannot be efficiently employed on account of the rapid destruction of the platinum points; and the method of obviating this difficulty by breaking contact under a cover of alcohol is only compatible with slow action. But by dividing the work amongst several coils, each with a separate vibrating contact breaker, this difficulty is avoided, and the frequency of the spark is multiplied in proportion to the number of coils and contact breakers. With these views, I obtained from Mr. Apps six induction coils, each capable of yielding a maximum spark of $10\frac{1}{4}$ inches. I had also a six-fold contact breaker constructed of the usual automatic vibrating type, and in which each vibrator was acted upon by an independent electro-magnet of the horse-shoe description. In experimenting with a single contact breaker used with a single coil, I found much advantage in augmenting the power of the working magnet, and in stiffening and shortening the spring greatly beyond the limits of ordinary practice. By so doing I was enabled to reduce the range of vibration, and thus to obtain with a heavy-headed vibrator many times the usual speed of oscillation without reducing the length of spark in nearly the same proportion. With an extremely high speed of interruption, I could obtain sparks 4 inches long in great profusion, though not with rapidity equal to the rate of vibration. To accomplish that degree of rapidity the sparking distance had to be reduced to about one-half; but, considering that a spark of 2 inches is supposed to represent nearly 95,000 volts when delivered between knobs, and would probably give two-thirds of that amount when delivered between points, I saw no sufficient inducement to strive after longer sparks, which can only be obtained by great sacrifice of frequency, involving a general reduction of ampèrage far exceeding the gain in voltage.

By using two coils in series with an alcohol contact breaker worked slowly, I could get sparks 15 inches long; but I found so many difficulties and inconveniences in a serial arrangement, and so little to be gained by it, that I abandoned the pursuit of it, and confined my attention to a combination in parallel. In the first instance I employed a secondary battery of seven large cells to supply the current for exciting my six coils, and I fully expected that their united output would be proportionate to their number, but in this I was disappointed. I found that two coils gave me only about one and a-half the effect of one, and that every additional coil gave a dimin-

ished increment of output. In fact, when all the six were in action, I only got about three times the output I obtained from one. It was some time before I discovered the cause of this apparent anomaly; but at last I traced it to the recoil currents from the condensers, which at each interruption of the primary current had to pass through the battery in the reverse direction of the battery current. Thus a conflict of currents was produced in the primary circuit, which checked the acquisition of magnetism by the coils. I saw no remedy for this interference, except the application of a separate battery to each coil, and I accordingly exchanged my single battery of seven large cells for six independent batteries composed of the same number of cells proportionately reduced in size, and when this was done I obtained the full measure of effect.

Side by side with the multiple contact breaker I have a mechanical contact breaker, in which the interruptions are effected by insulated cam wheels fixed on a revolving shaft, which is fitted with spur gear for high velocities. This mode of breaking contact has the advantage of causing the sparks to be delivered in regular sequence, with equal intervals between them. It also enables the exact number of discharges per second to be ascertained; but the automatic interruptor gives a larger output, owing, I suppose, to the fact that each break of contact takes place exactly at the moment when the magnetisation of the coil is matured, whereas with the mechanical break the same degree of coincidence cannot be attained. The six coils are placed vertically beneath an ebonite table, through which the connecting wires are conveyed in strong glass tubes, which also serve as pillars for supporting the sparking points. These points are adjustable for any required length of spark, and they operate in a radial direction against a metallic conductor surrounding an ebonite disc, which, by means of an india-rubber band and multiplying gear, can be very rapidly rotated; and, whether it be at rest or in motion, it serves as a collector, from which the united output of the coils can be drawn off. By rotating the wheel and conveying the current through a series of Geissler tubes fixed upon it in various positions, extremely brilliant symmetrical figures can be produced, but for all other purposes rotation is dispensed with, except to the extent of slight movements to regulate the length of spark without altering the adjustment of the points. A switch-board is provided for the purpose of regulating the number of coils to be thrown into action, so that they can be used singly or in any required number.

The power of this apparatus as indicated by the voltameter is much greater than that of the large hydro-electric machine made for the Polytechnic Institution, but it is very inferior to it in regard to length of spark, to which, however, I attach but little importance. I find it well adapted for experimental investigation, and I have obtained with

it some very interesting results. A drawing of it accompanies this paper (Plate 2).

My attention was at first directed to the heating effect of the secondary current at an air gap on its circuit. The amount of heat developed at this point when all the coils were in operation proved unexpectedly large, and I was surprised to find it almost entirely confined to the negative side of the gap. I found, also, that when the current passed in sparks very little heat was exhibited. It was only when the passage of the current assumed the appearance and condition of an arc that the heat came into prominence. Taking platinum wires of No. 27 B.W. gauge for the positive and negative terminals, which I shall call electrodes, to distinguish them from the sparking points at the collector, I found that the ends could be drawn asunder to a distance of fully an inch before a decided stream of crackling sparks was elicited, and even then the sparks presented a hazy appearance. On re-advancing the ends to each other the sparks diminished, while the haze increased and gradually assumed the condition of a pale blue arc. At a distance of 0·6 inch a well-defined arc could be maintained, though not without a slight admixture of faint sparks, which followed the curvature of the arc. At this distance the heat was sufficient to fuse the end of the platinum wire forming the negative electrode, but the heat did not reach its maximum until the separating distance was reduced to a few hundredths of an inch. At that small distance the negative platinum melted with great rapidity, and ran back in a globule until it got out of melting range of the arc. By following up the melted globule by steadily advancing the positive wire, the negative wire fused at the rate of nearly 3 inches per minute; but, however long this process was continued, the positive wire remained to all appearance perfectly cool. Retaining the positive wire unaltered, I increased by successive steps the thickness of the negative wire, to determine the point at which it became too thick to melt and run back in a globule, and it was not until I increased the thickness up to 16 gauge, which corresponds to a diameter of 0·065 inch, that I reached this limit. With that wire the end melted into a rounded form, but no longer receded in a gathering globule, as thinner wires did. Iridioplatinum wire of 21 gauge, containing 70 per cent. of iridium, readily melted and receded, though not rapidly, until the globule fell by its weight. In this instance the light emitted from the globule was so intolerably bright as to require darkened spectacles to view it with impunity. I next proceeded to ascertain how far it was necessary to reduce the thickness of the positive wire before it exhibited an equal degree of heat with the thickest wire opposed to it on the negative side, and I found that I had to diminish its diameter to 31 gauge before this equality was reached. Now, the sectional area of No. 16 gauge is 42 times

the area of No. 31 gauge, and from these figures an estimate may be formed of the difference in the development of heat on the negative and positive sides. When I employed two carbon electrodes of equal thickness the greater development of heat on the negative side was still very decided, though the difference was not so conspicuous as in the case of the platinum electrodes. In the arc lamp the superiority of heat is largely on the positive carbon, and it is difficult to account for the contrary result obtained in my experiments.

I further varied these experiments by taking the positive discharge from the surface of acidulated water, in which case the negative electrode was melted by the arc flame springing from the water. I also used for the positive electrode a lump of ice sprinkled with salt to make it conduct, and obtained the same result. A reversal of the current made the water boil at the surface and melted a hole in the ice, while the positive platinum remained unheated as usual. In another experiment I enclosed the two platinum electrodes in glass tubes sealed at the outer end, leaving about $\frac{1}{8}$ inch of the platinum wire (No. 27) projecting beyond the glass. These sealed ends I immersed in distilled water, and succeeded in melting the exposed portion of the negative wire while submerged in the water.

In another case I used an iron wire (No. 20 gauge) on the negative side, retaining the platinum wire for the positive electrode. In this instance the melted globule ran back very quickly to a distance of about $\frac{1}{2}$ inch, then stopped and burst into intensely brilliant flame, which showed a strong disposition to cross over to the opposite side, but appeared to be beaten back by the blue flame of the arc. Part, however, did reach the positive electrode, and condensed upon it in the state of an oxide. My attention having been thus directed to an appearance of conflict in the arc, I discarded the iron wire and substituted a platinum wire dipped in a brine of common salt, so as to impart a distinguishing yellow colour to any flame that might issue from the negative side in opposition to the flame emanating from the positive side. This caused the arc to be exhibited under two colours —a very decided yellow on the negative side, and the same pale hazy blue as before on the positive side, but the yellow flame was beaten back by the blue flame. Flecks of yellow could, however, be seen to get across occasionally. It was not so easy to produce yellow flame from the positive electrode, because there was not sufficient heat in the positive wire to volatilise the salt; but by using a separate wire encrusted with salt, and holding it alternately immediately in front of each electrode, I could produce yellow flame on either side and observe the difference of its behaviour in the two cases. When held on the positive side a dense unbroken stream of yellow flame supplanted the previous blue, and passed over bodily to the opposite wire; but when held on the negative side the yellow flame struggled

with comparative feebleness to cross over, and only reached the positive wire in scanty fragmentary portions. These appearances were strongly suggestive of a dominating force emanating from the positive side and opposed by a weaker force of the same nature in the opposite direction. The oscillations which are known to attend every disruptive discharge necessarily involve passages from the negative as well as the positive side. In fact the spark must be regarded as consisting of a dying out alternating current of prodigious frequency in which the positive alternations have the ascendency over the so-called negative ones; the excess constituting the available current.

Although the arc flame presented no appearance of flowing motion, I thought it necessary to test the question of any movement in the longitudinal direction by bringing the arc in contact with light powdered substances. For this purpose I placed a small heap of chalk dust upon a plate of mica, and caused the arc flame to pass through it, but none of the dust was moved lengthways. The displacement was entirely lateral, the material being neatly heaped up on each side in a ridge, which curved inwards towards the points of the electrodes, beyond which no displacement was visible. The action appeared to be that of a gentle push, rather than that of a sudden impulse; but when sparks passed instead of flame, a scattering force was exerted, which operated sideways, and not in the track of the spark. In this case I observed that the scattered dust showed a tendency to settle upon the plate in curved lines and symmetrical figures. I followed up this hint by sifting fine black dust upon white cardboard and passing sparks over the surface, and I thus obtained most unmistakable proof of symmetrical arrangement. This, however, was only a crude method of procedure, and it required a great deal of perseverance and innumerable trials before I succeeded in producing the effect in a satisfactory manner. At length, however, I was enabled to produce perfect dust figures which presented pictures of the disruptive discharge revealing the existence of forces of which the eye could otherwise take no cognizance. Many of these figures I got photographed on the spot, and a selection of them accompanies this paper. They represent the effect not of a single discharge but of a succession of sparks, generally beginning with a light one and gradually increasing the power as the dispersion progressed. To put all the power into one discharge had too much dispersive effect and produced great irregularity in the figure. A perfectly even sifting of the dust was also essential to a good result, and this could only be effected by the entire exclusion of air draughts and a regulated action of the sieve. The best kind of dust to be used and its proper degree of fineness were also matters requiring many experiments to determine. The dust which I ultimately used consisted of calcined magnesia worked up in a mortar with a suffi-

cient quantity of pure carbon to give a dark slate colour to the combined mass. The carbon was in the form of an impalpable powder, and, I believe, consisted of purified lamp black. I tried every variety of discharge I could think of—sometimes weak and sometimes strong—sometimes from a single coil and sometimes from several in combination. The most powerful sparks were obtained by using all the six coils in parallel and discharging them simultaneously by the action of a single alcoholic contact breaker common to the whole. I also used a Leyden battery, consisting of four ½-gallon jars, which I joined up in various ways both in parallel and in series, and in some cases introduced a wet string to soften the discharge.

The circular lines which surround the main discharge, both horizontally and transversely, first demand attention. They appear to represent sections of concentric layers or shells, of which the spark is the nucleus. Their great similitude to the lines of force represented by iron filings under the influence of a magnet is suggestive of similar causation, but I could not find any proof of their identity. The distance to which this circular action extended was far beyond the limit exhibited by the photographs. By laying patches of extremely light dust on paper at various outside distances, I distinctly traced these lines at a distance of 18 inches from the centre of dispersion, and there can be little question of the action existing in a lessened degree at a still greater distance. It can hardly be doubted that the particles of dust are linked together by polar attraction, and they probably represent similar conditions of the air jerked out by the discharge, but whether it be possible to regard them as indications of tracks of diffused discharge of a different character from that of disruption is more than I dare venture to say. I am satisfied that they are not mere ripples resulting from pulsatory vibrations of air, for I find that radial pulsations produced by mechanical means merely clear a circle without the least tendency to form similar rings. I made the experiment by stretching a membrane over a tin cylindrical box, with a small air tube leading from the bottom to near the surface of a dusted card. The membrane was then set in motion by a rapid succession of light taps, which dispersed the dust in the same manner as the spark did, but not the least indication of circular lines could be seen, and yet air puffs play an important part in the process, for the effects are greatly modified by screens, as will presently appear.

The figures also show in the most unmistakable manner that the wires, as well as the spark, exercise a dispersive force. I am inclined to think that the dispersive action of the wire differs only in degree from that of the spark. We see in the lines emanating from the wires evidence of a molecular disturbance in the material which shoots off the molecules of air in contact with the wire. When the

action is very strong, as it is near the sparking terminals, the external molecules of the metal are themselves shot off, as is evident from the fact that the positive wire becomes reduced to a neck a little in rear of the sparking end when subjected to a prolonged succession of Leyden battery discharges. It is also well known that if a discharge from a powerful Leyden battery is sent through a very fine wire, the whole wire is exploded. Under ordinary conditions the cohesion of the molecules restrains their movement within very narrow limits, and confines their action to mere impulse on the surrounding air. Now, it appears to me that we may regard the track of the spark as a line of conducting air, which, having no cohesion to keep its molecules together, is exploded at every discharge, and, consequently, produces a far greater amount of dispersion than the wire. The force* of the lateral discharge at short distance is very great. In all cases the dust beneath the conducting wires was struck into the card, so as to leave a permanent delineation of the wire. The dust also over all parts of the figure, where the action was strong, was forced into the card, so as to leave a stain after the loose dust was shaken off, and thus a stained picture of remarkable accuracy, embracing the greater part of the figure, was in some cases preserved.

In all the figures the emanations from the positive and negative wires are of the same character, but present a great superiority of power on the positive side. This superiority is, however, much greater in the case of coil discharges than in battery discharges. This may be attributed to the oscillations between the inside and outside coatings of the Leyden jars suffering far less degradation than those between the terminals of the coils. If there were no degradation, the forces exerted on the positive and negative side would be equal and everlasting. The application of wet string to the battery lessens the difference between these opposite forces, but it augments the lateral displacement of dust from both wires while it diminishes the explosive force of the spark.

Diagrams of the dust figures are annexed (Plates 2—13), but, though admirably drawn, they necessarily fall short of the photographs in showing the delicate lines and shadings of the actual figures. To suit the pages of the 'Proceedings,' the diagrams are reduced to about half the size of the originals, but the exact scale is marked on each. The following is a descriptive list of the figures:—

No. 1 was produced by a succession of sparks from six combined coils discharged simultaneously. The tracks of the sparks, the surrounding circular lines, the impress of the positive and negative

* In one case I knocked a large piece out of the bottom of a thick glass trough, containing only an inch depth of water, by discharging an under-water spark along the bottom.

wires, the strong emanations from the positive wire and the feeble ones from the negative, are shown in this figure, as well as in most of those which are to follow.

No. 2 may be regarded as a transverse section of No. 1, being produced by similar sparks delivered vertically through a hole in the dust plate when fixed horizontally midway between the sparking points. It shows that the discharge is surrounded by circular lines in the transverse as well as in the longitudinal direction.

No. 3 is a similar transverse section taken a little in front of the positive point.

No. 4 is the same thing taken immediately in front of the negative point.

No. 5 is taken transversely immediately behind the positive point, and shows the radiations issuing from that part of the wire.

No. 5A shows similar radiations from the positive wire at a distance of about 12 feet in rear of the sparking point.

No. 6 was produced by a succession of discharges from four ½-gallon Leyden jars joined up in pairs, two in parallel and two in series. It will be observed that the circular lines are much more strongly developed than in the preceding figures, but that the radiations from the wires are less so.

No. 7 was produced in the same manner, and shows the circular lines still more highly developed.

No. 8 is the transverse section of No. 7 taken in the same manner as No. 2.

No. 9 is the same as No. 7, except that the conducting wires are brought down upon the card at a steep angle.

No. 10 was produced with battery discharges in the same manner as No. 6, but with a short wet string introduced to soften the discharge. This had the effect of greatly increasing the radiations both from the positive and negative wires, while it reduced the development of the circular lines.

No. 11 was produced under the same conditions as in the preceding case, except that the wet string was considerably lengthened.

No. 12 was similarly produced, but with a still further increase in the length of the wet string.

No. 13 was produced without any visible discharge across the dust plate. The same battery was used as in the last and several preceding cases, but the Leyden jars were allowed to leak sufficiently to prevent their reaching the sparking point. The same effect may be produced by taking the discharge at a by-pass with a shorter sparking gap than that on the dust plate.

No. 14. In this case the battery was discarded and sparks only from the combined coils were used, the same as in No. 1. It shows the effect of splitting the positive current by the use of a double wire

re-united at the sparking point. The double wire is bent into opposing bends and angles to show the repellent action of the radial emanations from the wires. The resting places of the dust are very beautifully shown by the darkened spaces in this figure.

No. 15 was produced in the same manner as the preceding, but on glass instead of card. As the dust is so very easily moved on glass, only one coil was used, and it is remarkable that the dispersive effect exhibited by the double positive wires is actually greater than that of the spark itself. It will be seen that the dark impress of the wire is more marked upon the glass than upon the card. It will be observed also that there is a very peculiar dark band lying outside of each wire and running parallel with it, and that each of these bands merges in a dark patch lying on each side of the sparking point. These bands and patches may be assumed to represent places where the dispersive force is considerably subdued.

No. 16 is a transverse section of battery discharges showing the deflections of the circular lines produced by the interference of six glass tubes $2\frac{1}{2}$ inches long and $\frac{5}{8}$ inch in diameter, erected on the dust plate at equal distances from the centre and from each other. It will be observed that the lines are not obliterated behind the tubes and that curious new curves are developed.

No. 17 shows similar effects produced by heavier discharges on a more thickly covered dust plate. In this case it will be seen that the circular outline of the figure is changed by the operation from a circular to a twelve-sided form. A blunted angle is thrown out opposite each glass tube and another midway between every two.

No. 18 shows another dust plate similarly treated, but more lightly covered and without the glass tubes, instead of which two flat screens of cardboard, 3 inches high and 2 inches wide, were fixed perpendicularly on opposite sides of the centre. In this case, although the lines curve inwards behind the screens, they gradually die out towards the centre and leave the middle portion undisturbed, but by reducing the height of the screens to the level of the sparking point the whole sheltered space became wholly filled up with lines as in the two previous cases.

No. 19 shows the effect of inverting two wine glasses upon the dust plate so as to cover two circular patches of the dust and protect them from the action of the air. In this case no lines were found within the glasses.

No. 20 shows a stained figure remaining on the card after the dust was shaken off. Although there are some small portions where the stain has failed to take effect, the figure is, upon the whole, preserved with remarkable accuracy.

No. 21 is another example of a stained figure in which some of the circular lines are discernible.

[No. 22 shows a circular barrier of six wooden hemispheres, each $1\frac{1}{2}$ inches diameter, and touching one another, formed round the centre of the dust plate, and the dust was swept off the inner space before making the experiment. The positive sparking point was level with the top of the hemispheres, and the discharges were delivered from a Leyden battery of two $\frac{1}{2}$-gallon jars in series. It will be seen that the whole of the dust outside the barrier is thrown into lines which form arches over each touching point of the hemispheres, and that the spandrils are filled up with inverted curves. Although it is not easy to see how these arches and curves can be attributed to eddies, yet appearances favour the view that the lines are due to the combined effect of obstructed air drift and electric polarity. A barrier formed by a continuous perpendicular screen of the same height as the hemispheres almost entirely prevents the formation of lines.—*June* 9.]

Reverting to the subject of the development of heat at the negative side of the arc, the question arises, from what source is the heat derived? It cannot be acquired by conduction from the flame, for mere conduction would heat both wires alike, nor can it be the result of convection, for the arc is stagnant in the longitudinal direction. The only explanation I can see is that the negative wire requires time to take up the sudden gushes of current that come over from the opposite side, and which, not being instantly carried onward, produce a tumultuous agitation of molecules at the receiving end of the wire, resulting in the manifestation of heat. The thicker the wire the greater the facility of passing on the current, and hence less heat is evolved in a thick than in a thin wire. On the other hand, the positive wire receives comparatively small returns, and therefore a relatively thin wire suffices to pass on the pulsations without its being heated to ignition. But the question remains, why is it that so much less heat is produced by the spark discharge than by the arc discharge? Probably the chief reason is that the spark represents less quantity of current, though higher in potential. Another reason may be that the spark expends more energy in mechanical disturbance than the arc. Then, again, there is the question, what is the relation between the spark and the arc? I cannot find that the spark is possessed of mechanical impulse in the direction of its length any more than the arc. Both produce lateral but not longitudinal dispersion. I have discharged a quick succession of powerful sparks in a downward direction so as to pierce a piece of thick cardboard suspended from a delicate balance-beam, but without effecting any decided disturbance of the balance. Every spark left a burr on each side of the card apparently equal in size, which alone is sufficient to show that the spark does not pierce like a needle. Probably the arc and the spark are of much the same nature, the spark

being a single act of discharge and the arc a multitudinous succession of minutely divided sparks, of which none are sufficiently strong to produce any violent disturbance of the adjacent air. The hissing sound emitted by the arc seems to favour this view.

I now come to another set of experiments, which will only require a brief notice. They were designed to show the effect of passing both the arc and the spark through an intervening combustion flame.

Speaking generally, the intervention of flame has much the same effect in increasing the length of the disruptive discharge, whether in the shape of arc or spark, that is effected by rarefaction of air; but I will here only particularise some curious effects I obtained with the flame of paraffin candles. The annexed series of illustrations exhibit the effects observed in each case.

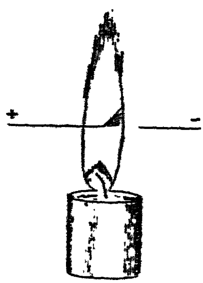


Fig. 1.

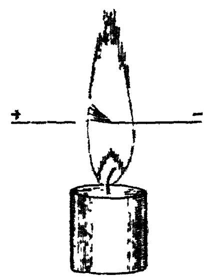


Fig. 2.

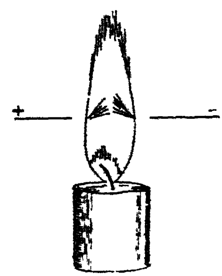


Fig. 3

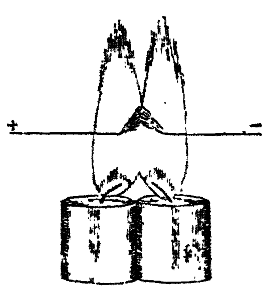


Fig. 4.

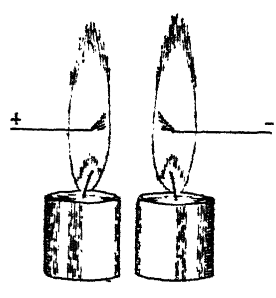


Fig. 5.

Fig. 1 shows the positive wire immersed in the flame and the negative wire clear of it, both wires being of platinum and of 22

gauge. A brilliant jet of pale-yellow flame was projected from the positive wire as shown, but did not go beyond the candle flame. Nothing was to be seen in the interval between the flame and the negative wire, which, nevertheless, heated as usual. It is quite possible, however, that there might be an arc communication rendered invisible by the intense brilliancy of the jet within the candle.

Fig. 2 shows the negative wire immersed in the flame and the positive clear of it. Here the direction of the jet was reversed, and the negative wire, instead of heating, became covered with a clot of carbon; but with a thinner wire the negative did heat, and no clot of carbon was formed.

Fig. 3 shows the effect when both wires were clear of the flame. In this case two jets of brilliant light appeared in the flame with a slight separation and an appearance of conflict between them.

Fig. 4 shows two candles brought together so as to form one broad flame in which both wires were immersed. In this case a large clot of carbon accumulated on both wires, and neither of them heated. An examination of the carbon showed it to be deposited in very beautiful florescent and fern-like forms. It would have quickly filled up the whole interior of the flame had flakes not fallen off as the mass grew in size.

Fig. 5 shows the two candles drawn asunder so as to leave a small vacant space between them. Nothing could be seen to pass this dividing space, but the results remained the same as in the previous case.

The repellent action which is exhibited in all these cases is not easy to explain, seeing that the discharge on both sides seems bounded by the exterior of the candle flame.

I am at present continuing my experiments, but under the altered conditions of discharge in rarefied air. As these experiments are far from complete, I must reserve them for a future communication.

I must not, however, close this present paper without referring to an experiment of quite a different type from any of the preceding, and which was made long ago with my large hydro-electric machine. In that instance two wine glasses, filled to the brim with specially-distilled water, were placed in juxtaposition, leaving only a space of about $\frac{1}{4}$ inch between them. A long cotton thread was then coiled up in one glass and the upper end of the thread dipped into the water of the other glass. When the steam was turned on the thread was drawn out of the glass in which it was coiled and conveyed with great rapidity into the other glass, and for a few moments a rope of water remained suspended between the lips of the two glasses. It was only when the machine was at its maximum power that I could do this, and it never reached its highest power when used within the

London building; but with my multiple machine I have succeeded in reproducing the experiment in a modified form. Taking two glasses of the form shown in fig. 6, placed near to each other as in the original experiment, I inserted in one of them a block tin cup in the manner shown in the figure. Into this cup I coiled a string composed

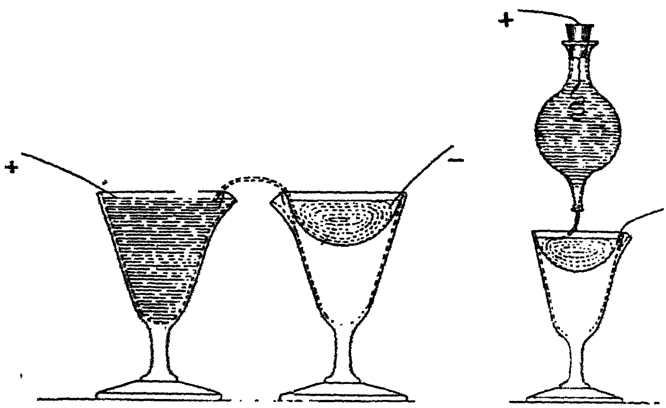


Fig 6. Fig. 7.

of ten strands of fine lamp cotton, which, when laid together, were of a sufficient section to make an easy fit for a hole of $\frac{1}{8}$ inch in diameter. The negative wire was put in contact with the tin cup and the positive wire was inserted in the other glass. The upper end of the cotton string was then laid over the edges of the two glasses, and the tin cup and the positive glass were then each filled with chemically pure water up to the same level. When the current was turned on the cotton string commenced to crawl over the edges of the two glasses, and never ceased to travel until it was bodily transferred from the negative cup into the positive glass. It was natural to expect that the water with which the cotton was loaded would travel with the cotton and raise the level of the water in the positive glass above the level of that in the tin cup, but the contrary was the case. The water lowered in the positive glass and overflowed in the tin cup, the surplus descending into the empty space beneath the cup, where its quantity could be seen and estimated. In short, the cotton travelled one way and the water the other, notwithstanding that the flow of the water was in opposition to the motion of the cotton. No spark passed between the glasses until the tail of the cotton ceased to touch the water in the cup, and then sparks passed in profusion. When the cotton was restrained from travelling, the water came over from the positive side in greater abundance, but when the cotton was dispensed with, and the gap between the vessels was bridged by a

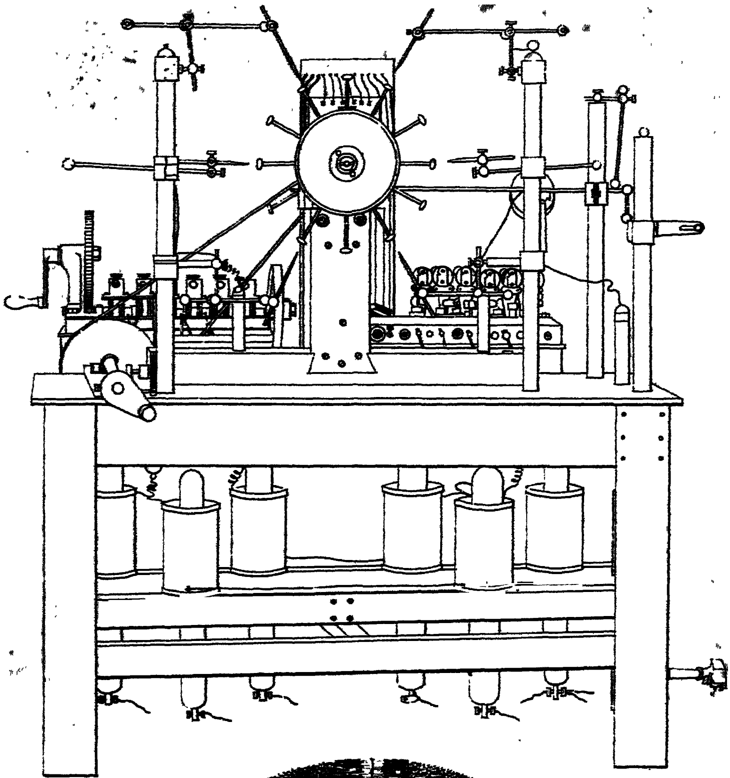
siphon glass tube, the water entirely ceased to pass. I am therefore led to believe that the intervention of capillarity is essential to the production of the effect.

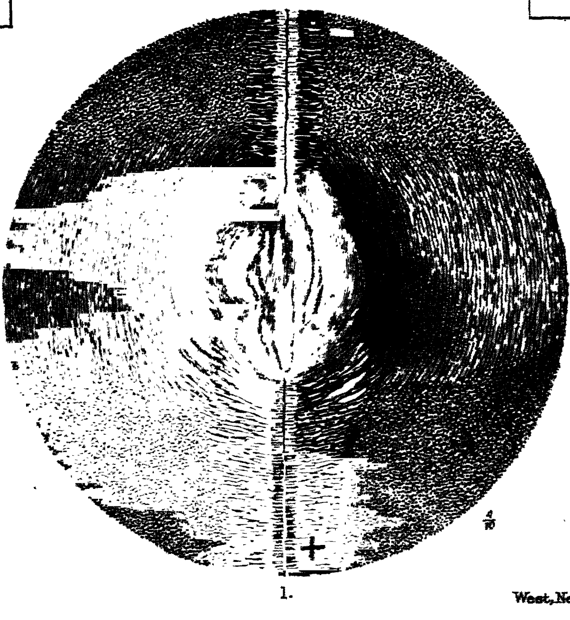
The tin cup is not essential to this experiment, and was merely used to afford a separate lodgment for the water brought over from the opposite side, and at the same time make the best possible contact with the water it contained.

Fig. 7 shows this experiment in a still more striking form. In this case the positive glass was dispensed with, and the upper end of the cotton coil was passed into a glass bulb through a nozzle, the aperture in which was just sufficient for the cotton to fill without appreciable friction in moving. A knot was made at the end of the cotton to prevent its dropping back through the aperture, and water was then poured into the bulb until nearly full, and a cork, with the conducting wire through it, was inserted. The bulb was held over the metallic cup with the nozzle dipping into the water. When the current was turned on the cotton climbed up vertically, and continued to pass until it filled the bulb. Even when the nozzle was lifted a quarter of an inch clear of the water the cotton continued to travel. The smallest particle of salt added to the water, or a minute quantity of anything that increased its conducting power, destroyed the effect and caused gas to appear at the electrodes.

The only experiment that I know of that presents any analogy to the effect thus obtained is that which appears to depend upon what is called electrical endosmose, in which case a porous diaphragm has the power of transmitting water from a cell containing a positive electrode into an adjoining one containing a negative electrode. If we may assume the capillarity of the cotton to represent the porosity of the diaphragm, it is reasonable to suppose it capable of transporting the water. Then as to the cotton travelling in the reverse direction, that may possibly be due to the reaction attending the transmission of the fluid. At all events it would appear that capillarity is controllable by electricity, and the question arises, what is the relationship between the one and the other?

The difference in the action of the current in this experiment and in that which I made with the hydro-electric machine consists in this:—That in the early experiment one thin cotton thread was moved with a high velocity, while in the latter experiment a mass of cotton equal to more than 100 similar threads was moved with a low velocity. This difference is probably due to the current being continuous in the one case and pulsatory in the other, also to its being of higher potential and smaller volume in the hydro-electric than in the multiple machine. When I attempted to use a single fine thread with my present machine the water upon it immediately boiled, and the thread was destroyed by sparks.





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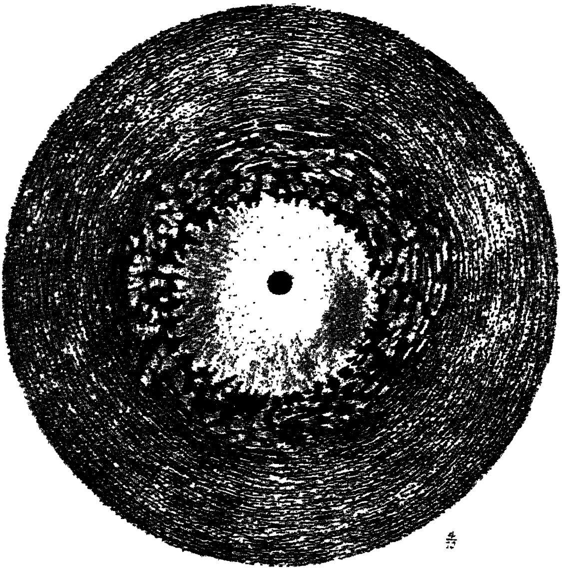

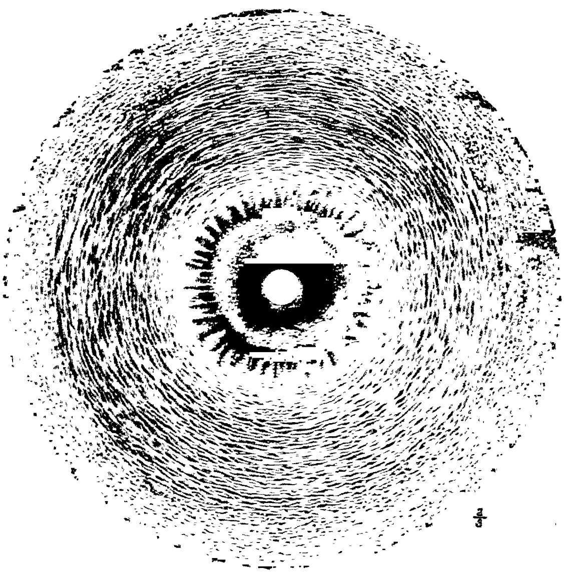

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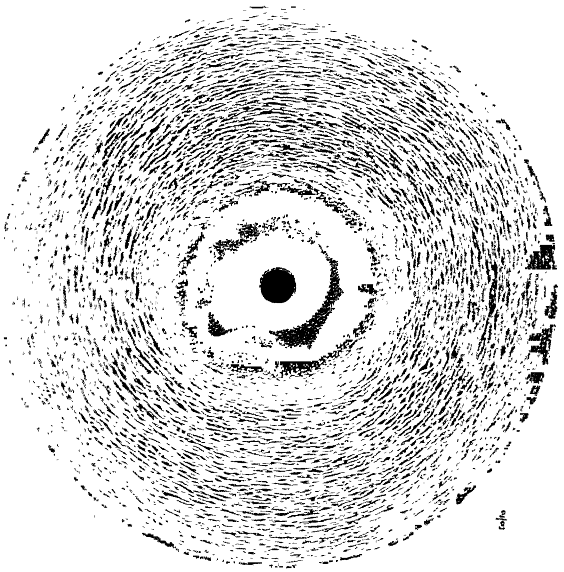

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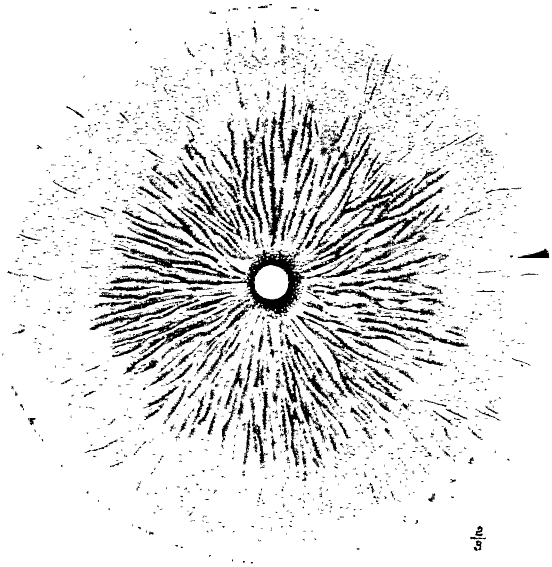

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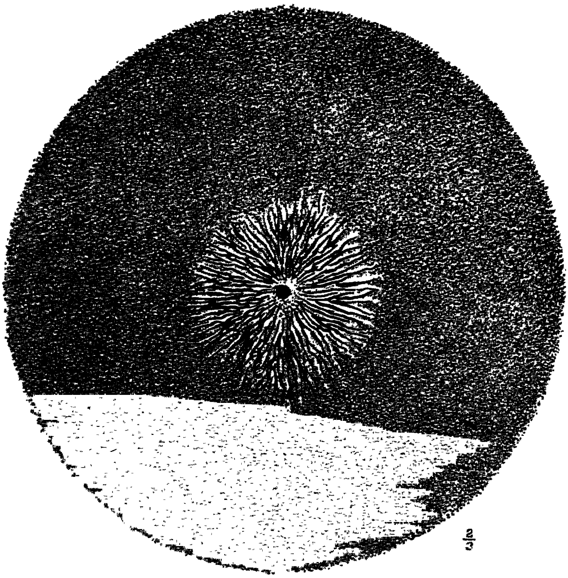

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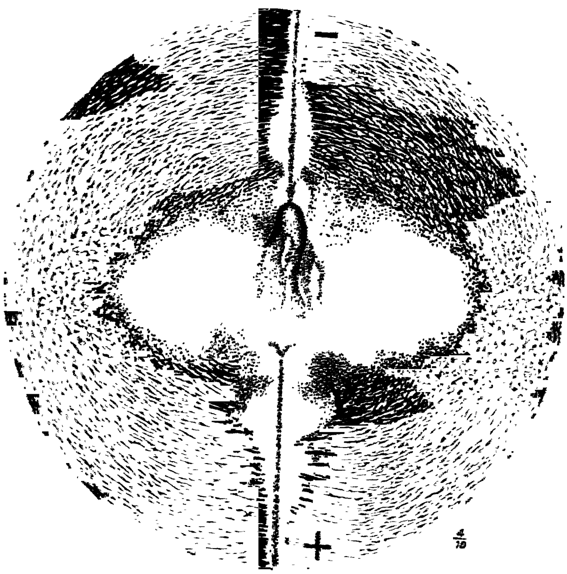

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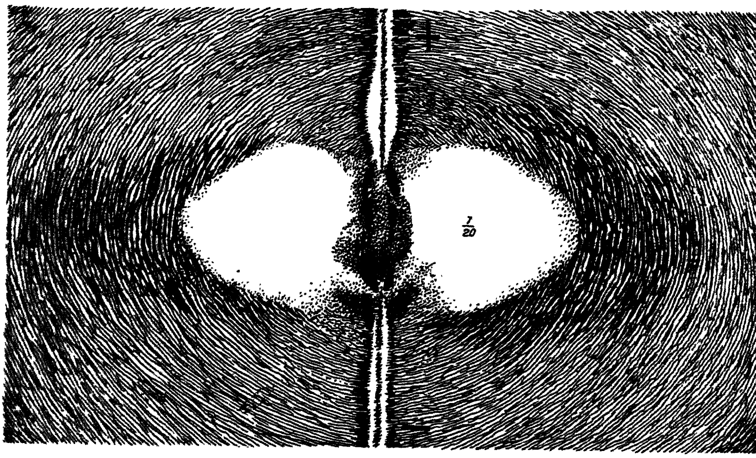

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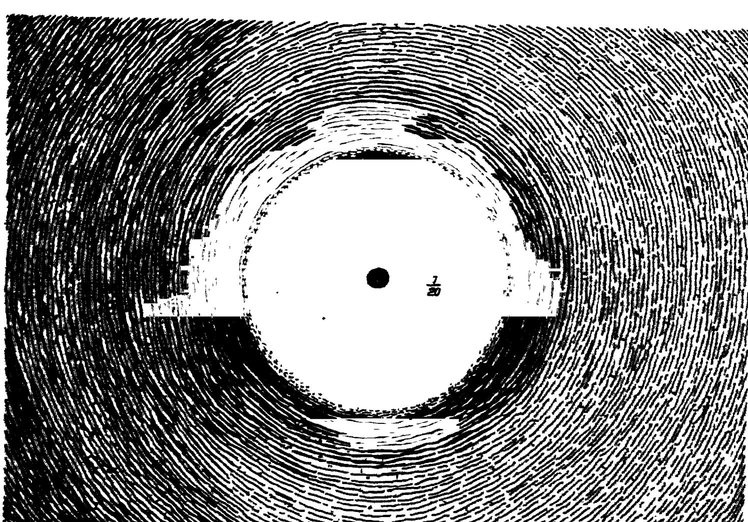

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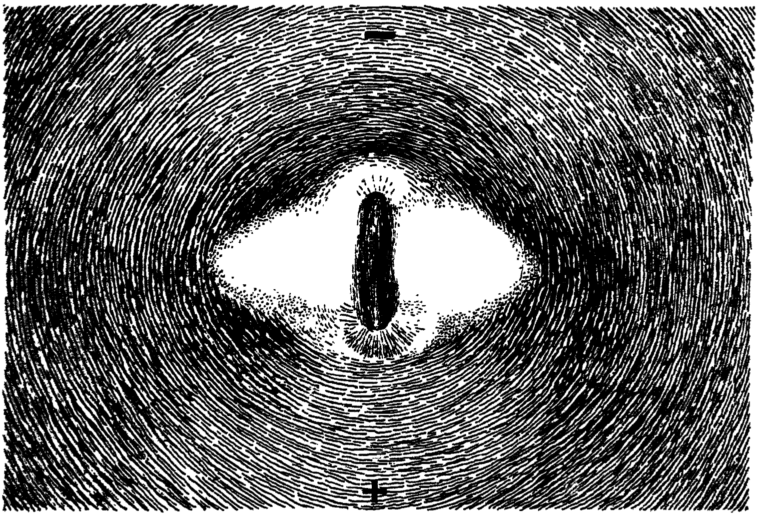

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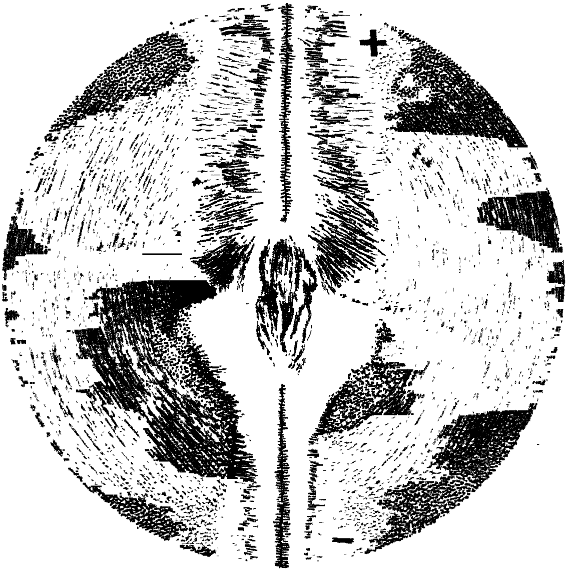

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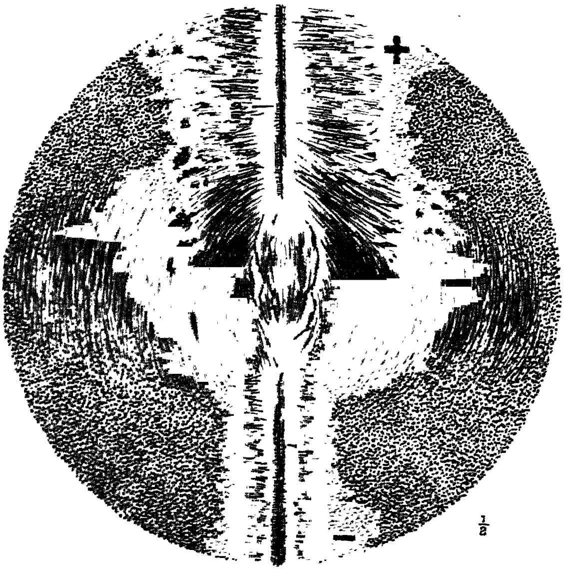

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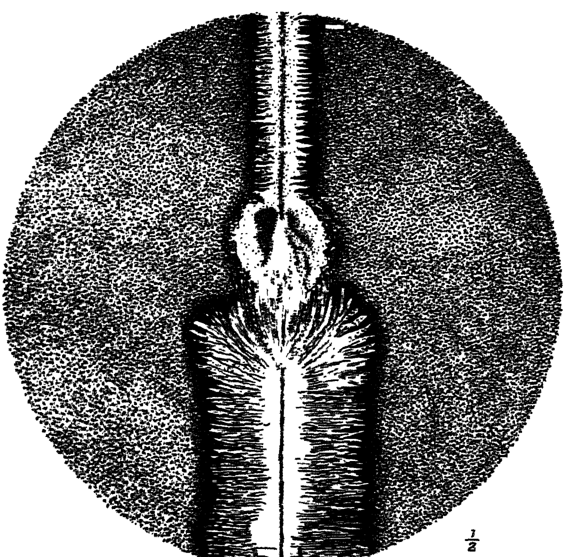

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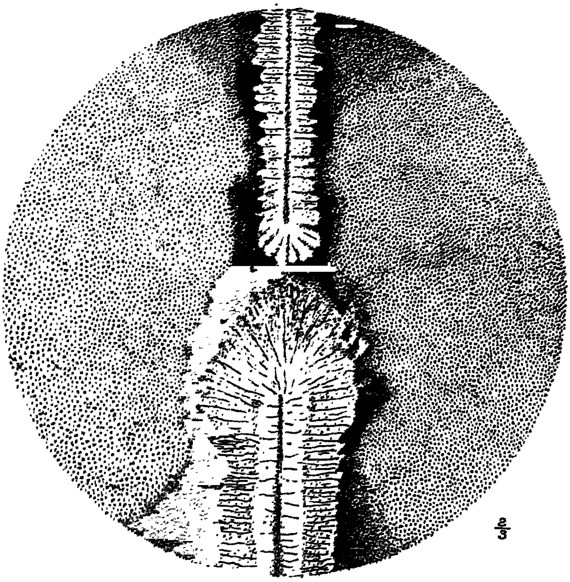

$\frac{2}{3}$

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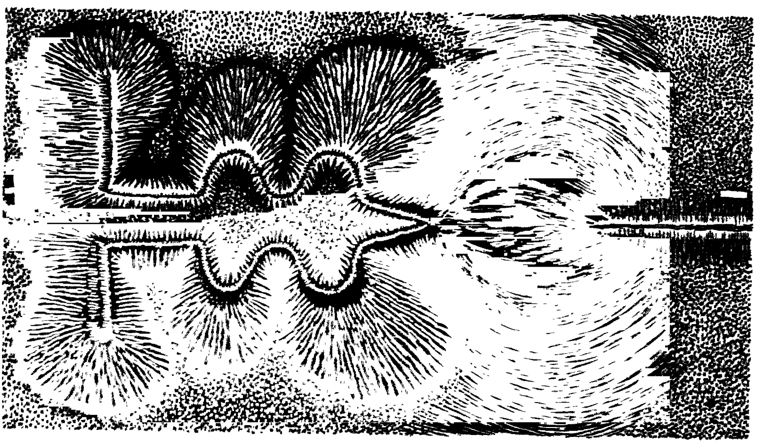

14. $\frac{1}{2}$

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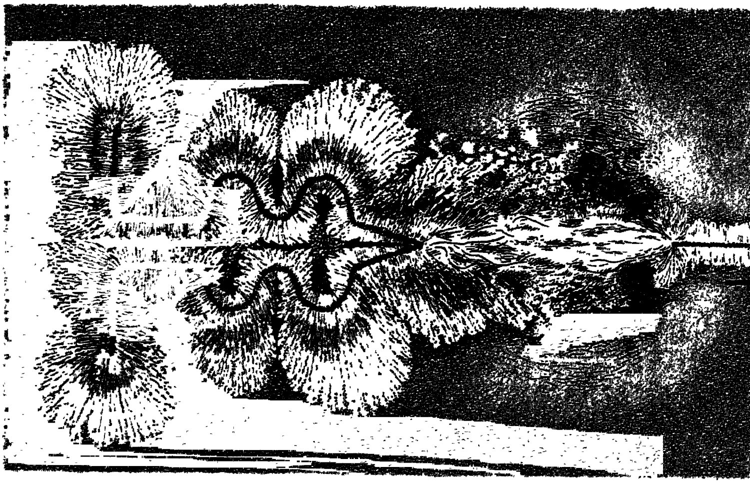

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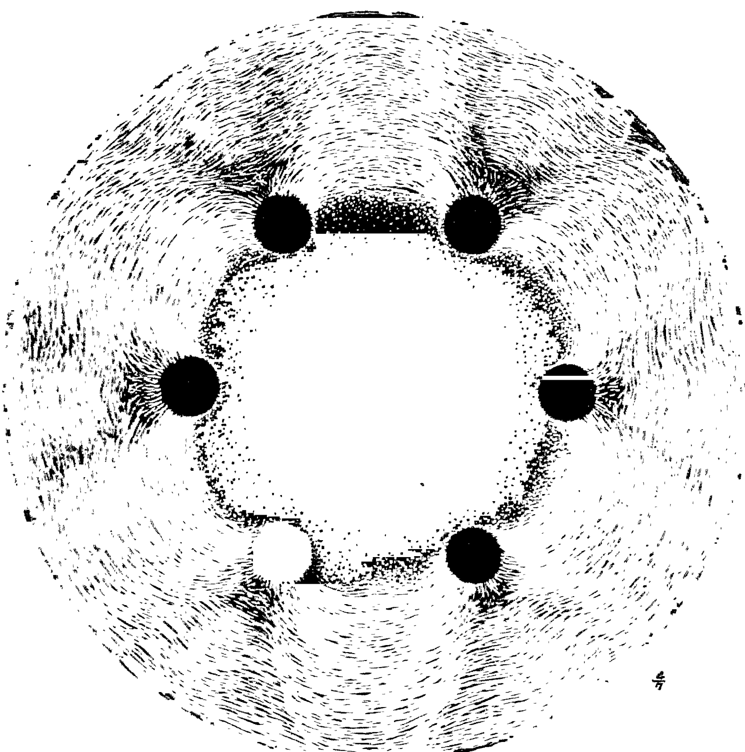

$\frac{4}{7}$

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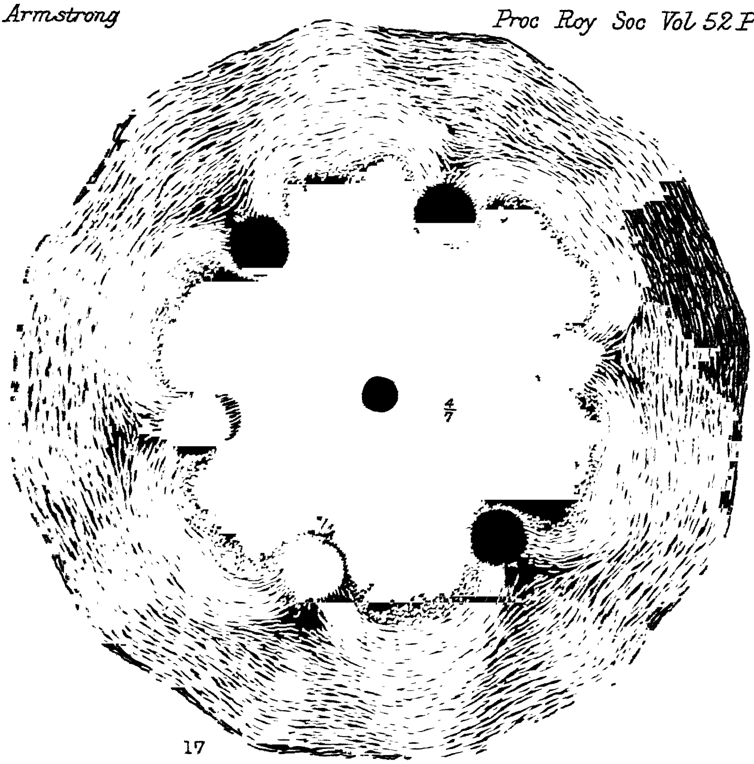

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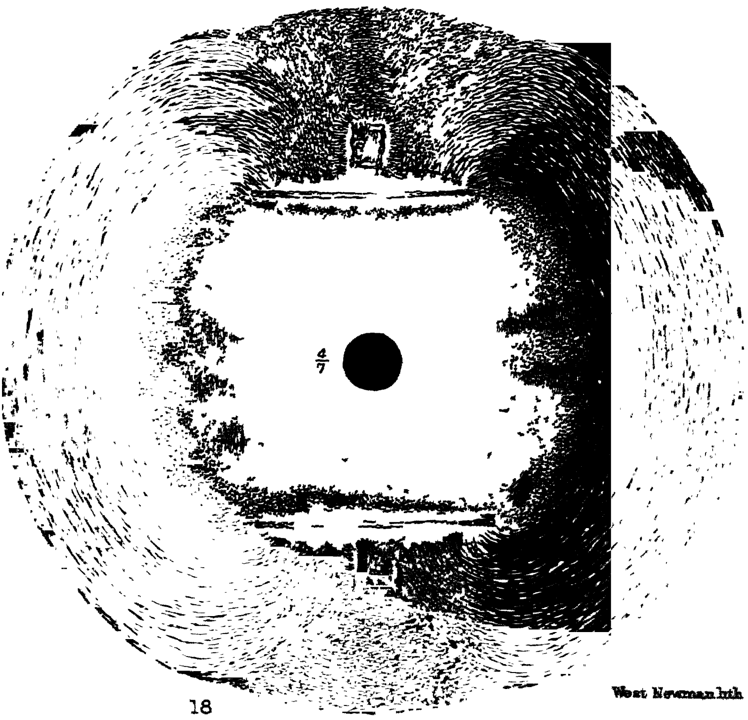

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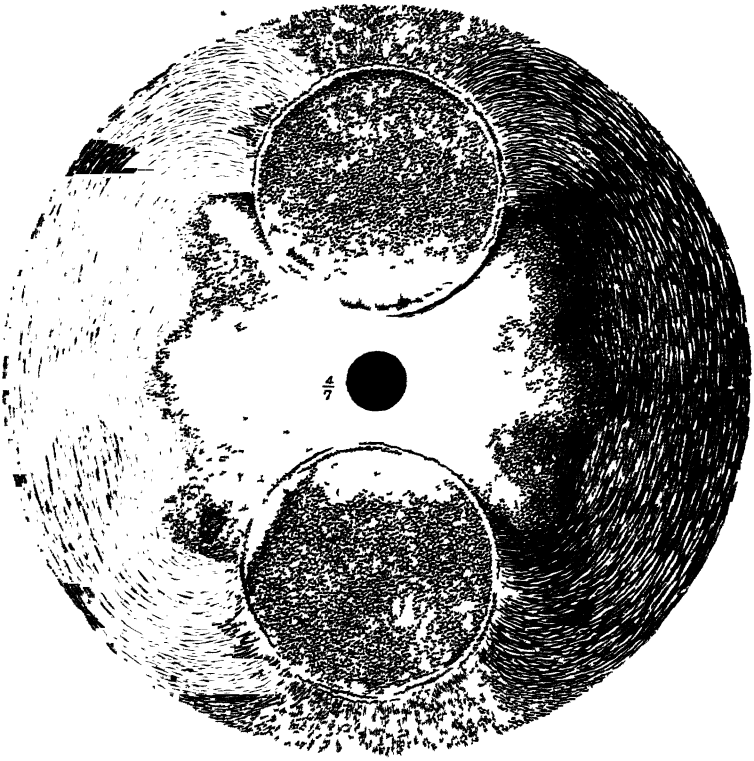

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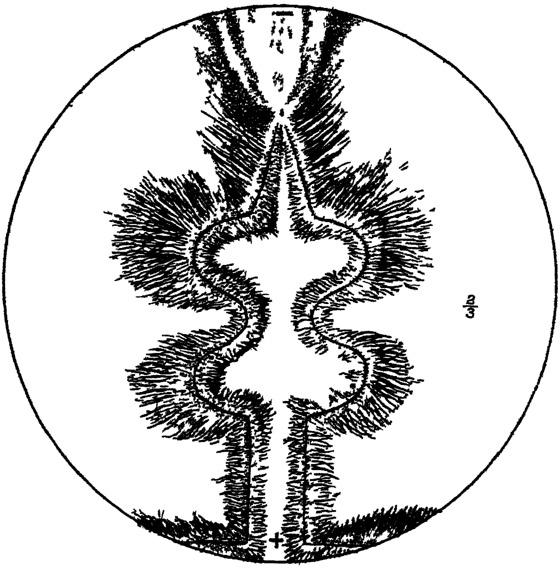

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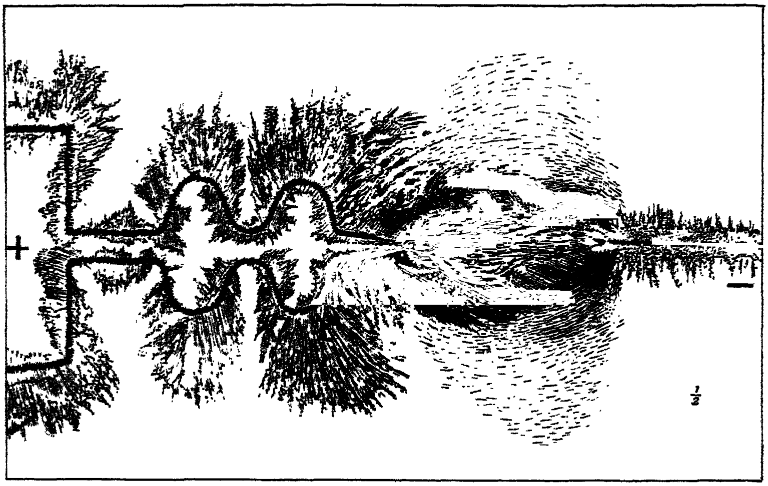

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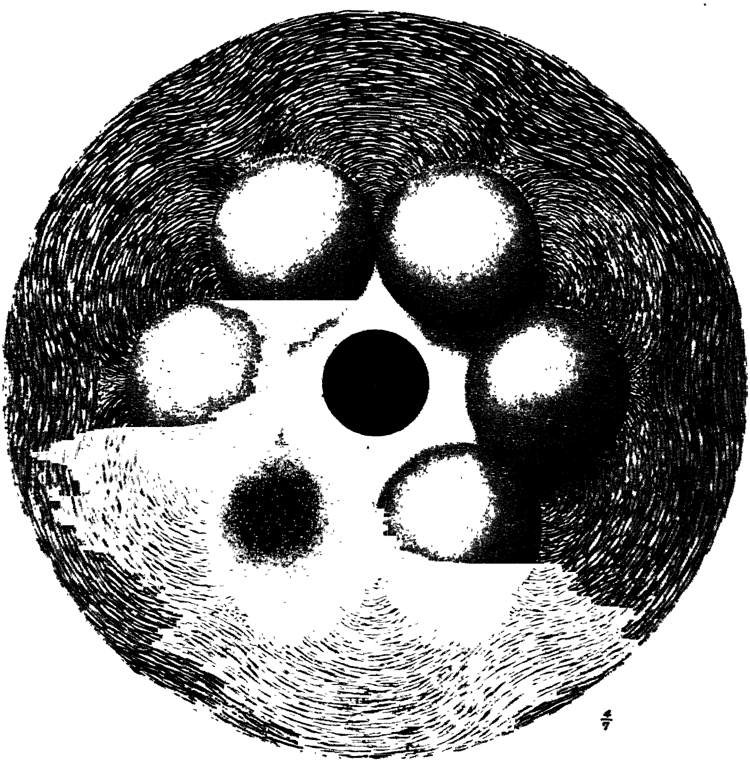


22.

West, Newman lith.

My acquaintance with the literature of electricity is very limited. Others vocations have for many years diverted my thoughts and my reading from electrical science, and if I have in any case advanced as a novelty what has been done before I hope it will be attributed to ignorance and not to intention. It was only within the last few days, and after the foregoing paper was printed, that I was made aware of the fact that I am not the first to use dust figures in electrical research, but I am still unaware that the experiments I have described have, in their results, any substantial coincidence with the recorded results of other investigations.*

"On the Simultaneity of Magnetic Variations at different places on occasions of Magnetic Disturbance, and on the Relation between Magnetic and Earth Current Phenomena." By WILLIAM ELLIS, F.R.A.S., Superintendent of the Magnetical and Meteorological Department, Royal Observatory, Greenwich. Communicated by W. H. M. CHRISTIE, F.R.S. Received April 7,—Read May 5, 1892.

The observations made in a magnetic observatory usually include absolute measures of magnetic declination, horizontal force, and dip (inclination); with photographic registration of the variations of declination, horizontal force, and vertical force; to which is added, at Greenwich, a photographic registration of earth currents.

As regards magnetic variations, the observations made at the Royal Observatory indicate, for all magnetic elements:—1. A progressive change of value which, when limited periods of time only are considered, is nearly constant from year to year. 2. A solar diurnal variation, the amplitude of which is greater in summer and smaller in winter, and which has also a period sympathetic with the sun spot period, being greater throughout the year when sun spots are numerous, and smaller when sun spots are few. There is also a small lunar diurnal variation. 3. The occurrence of days and periods of irregular magnetic disturbance or magnetic storm, which are more frequent and of greater magnitude when sun spots are numerous than when sun spots are few, being comparatively rare as well as insignificant in character near to the times of minima of sun spots. Disturbances are also in general more numerous in spring and autumn, than at

[* The actual operation of producing dust figures by the discharges of an induction coil was exhibited by the author at the conclusion of the reading of his paper. Two figures were produced, the one agreeing with diagram No. 15, and the other, a new figure, obtained by arching the positive wire and causing it to touch the plate at a succession of points.]

other parts of the year. 4. Earth currents, which, usually feeble, are active and strong at the times of magnetic disturbance, remaining active only so long as disturbance continues. In the absence of magnetic disturbance there exists practically earth current calm.

These are the broad features of magnetic variations as experienced at Greenwich, and which there is reason to believe are generally similar at other places.

Having premised thus much, we will now proceed with the special inquiry to which this paper is directed.

When magnetic disturbance is experienced at Greenwich, the photographic registers show that, after a period of magnetic calm, disturbance commences sometimes very suddenly, and at other times with a premonitory sign or signs. In the latter case the first indication will not unfrequently be a sudden and sharp movement occurring simultaneously in all elements, moderate it may be in amount and of isolated character, followed after a shorter or longer period by general magnetic disturbance; at other times the first sharp movement ushers in at once the disturbance. In other cases disturbance will arise gradually without any special premonitory indication, appearing in one element before showing, in any marked degree, in the others. But when there is sudden initial movement, whether great or small, it is very definite in character, and appears at Greenwich without exception simultaneously in the registers of declination, horizontal force, vertical force, and earth currents. The *instantaneous* movement is the really remarkable point. Much larger motions will occur during the course of a magnetic disturbance, but not usually movements similarly sudden. They are such as will be quite familiar to all those who may be acquainted with photographic registers of magnetic variations, and we shall see that similar characteristics are observed at other places.

Now it is known that any considerable magnetic disturbance or magnetic storm is felt over wide areas of the earth's surface, commencing and terminating at different places at about the same absolute time. But how nearly such commencement is really simultaneous at different places, whether or not in a much closer degree than has before been supposed, does not appear to have hitherto been so carefully studied as seems possible. For the initial movements are so definite and so suddenly manifested that it is evident, from the character of the photographic traces at Greenwich, that at such times a very perceptible, sometimes the whole, movement has occurred in a very few seconds, the fineness and exceeding delicacy of the photographic trace, as compared with that of the ordinary register, showing how rapidly the spot of light has moved across the paper. The accidental comparison of the Greenwich motions with those at other places in one or two instances of the kind seemed to show that the corre-

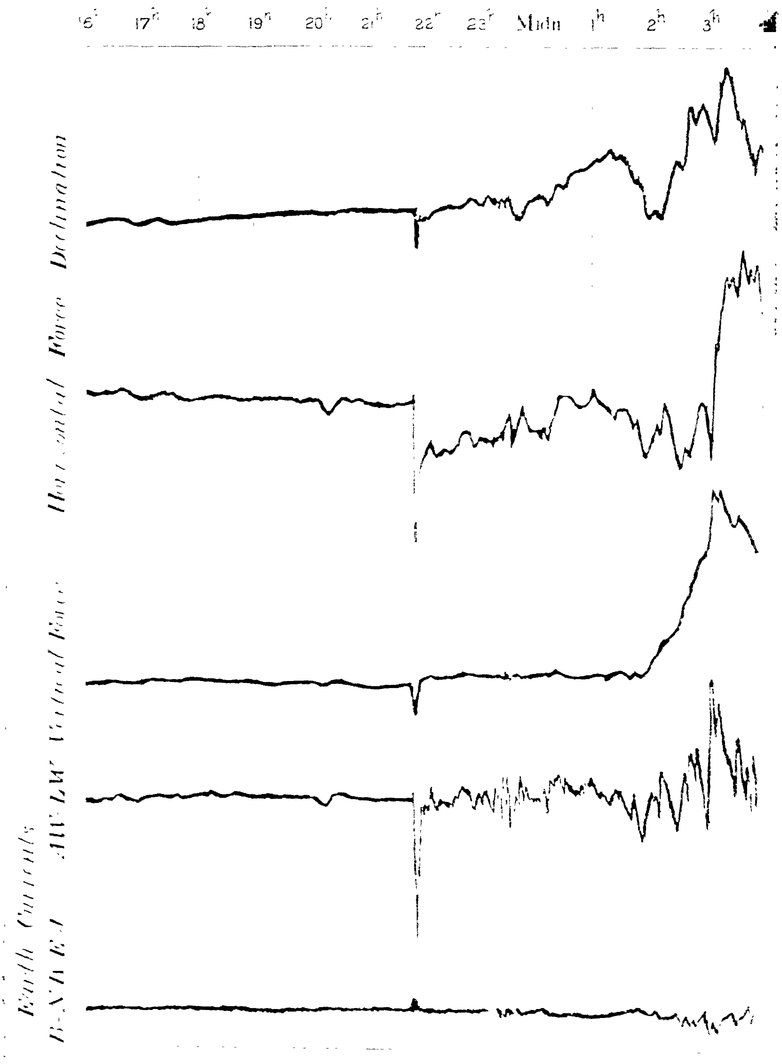
sponding phenomena at other places were in these cases similarly definite. It thus appeared that a systematic comparison of the times of these initial movements at different places might afford the means of determining how nearly the movements at such places are really coincident.

It is practically impossible to catch these first movements by eye observation of the magnets, from the uncertainty as to the time at which they may occur, so that it becomes necessary to rely on the photographic register. Unfortunately, however, the scale of time employed on such registers is necessarily small, one minute of time corresponding usually to about 0·01 inch. This renders difficult any very accurate measurement of time in individual cases. But it appeared to me that by selecting a considerable number of instances of sudden initial movement, and comparing together the times as measured from the photographic registers of magnetic observatories situated in widely different regions of the earth, we might expect in the general average to eliminate any accidental error of measurement due to contracted time scale or other cause, and, supposing no systematic error to exist, so obtain some reliable information as to the degree in which, on the whole, such movements, at different places, are or are not simultaneous.

With this object in view, a selection from the Greenwich registers was made of seventeen cases of sudden distinctive movement, preceding or commencing magnetic disturbance, and occurring during the years 1882 to 1889. The list might have been extended, but the cases selected are typical, and seemed sufficient for the purpose of a preliminary inquiry. Giving the day and approximate hour only of the occurrence of the respective movements at Greenwich, the directors of various magnetic observatories were asked to examine their registers for the corresponding times, and see whether there existed any instance of sudden movement, and, if so, to measure out and supply on each day, and for each magnetic register, the exact local time of commencement of movement to the nearest minute of time, and also the direction and approximate amount of change in each magnetic element. The observatories selected, including Greenwich, nine in number, were, counting from west to east, those of Toronto, Greenwich, Pola, Pawlowsk, Mauritius, Bombay, Batavia, Zi-ka-wei, and Melbourne. This request was complied with in the most obliging manner, and my best thanks are due to the directors of the various establishments for the very kind way in which they endeavoured to supply all necessary information, as well as to the Astronomer Royal for permission to make use of the Greenwich records. In two cases, indeed, Toronto and Zi-ka-wei, copies of the registers were also furnished; the Toronto report, however, included movements of the declination magnet only. Unfortunately at Pola

photographic registration was not commenced until the year 1885, and it thus became necessary to omit Pola in the comparision. It might be supposed that, in giving the Greenwich time of movement to the nearest hour only, it would be possible in some cases to find on the registers at other observatories several movements sufficiently near, any one of which might be taken, and thus not be a corresponding movement in the sense meant. But the movements in question, so abrupt in character, are neither numerous nor easily mistaken. Their character is well illustrated in the annexed reduced copy of the Greenwich registers for 1884, October 1 (one of the selected days), in which it will be perceived how in all elements, declination, horizontal force, vertical force, and earth currents, a definite and rapid movement occurs just before 22h., and simultaneously in all; that in horizontal force corresponding indeed to an increase in the earth's horizontal force of about 1/200th part. It will be thus understood that the really corresponding movements would be readily identified at other places; indeed the returns received show that no difficulty was experienced, the suddenness of movement being a matter of common remark. For instance, at Toronto it is said that "the initial movement was always very sudden," at Mauritius the movements are spoken of as "remarkable jumps," at Bombay as "abrupt and unmistakeable at the beginning," and at Batavia the commencement was "in all cases . . . . . very abrupt." In the Zi-ka-wei curves, so kindly furnished, the same characteristics are to be observed, and the same simultaneous commencement of movement in all elements is shown as in the Greenwich specimen register of 1884, October 1. The certainty with which the selection was made further appears by the circumstance that the stations in addition to Greenwich being seven, and there being seventeen days, 119 identifications had to be made. Not counting six instances in which the corresponding record was not available on account of failure or want of register, it was found that, of the remaining 113 cases, there were only four in which there was discordance, and a re-examination of the registers in two of these cases immediately showed that a wrong movement had been taken. Had there been opportunity for further examination in the two remaining cases, there is little doubt but that the source of the discordance would have been in these also ascertained. All this assures us of the real correspondence of the movements discussed.

It has been mentioned, in regard to Greenwich and Zi-ka-wei, that the three magnets, declination, horizontal force, and vertical force, were on all occasions simultaneously affected. The reports received from other observatories show that, at places for which the times for the different magnets are separately given, the magnets were similarly simultaneously affected. This circumstance is peculiar to



Royal Observatory, Greenwich.—Copies of the Photographic Records of Magnetic Elements and Earth Currents from 1884, Oct. 1, 16h, to Oct. 2, 4h, Greenwich Civil Time.

It will be seen how the rapid movement just before 22h occurs simultaneously in all elements, after a calm state. Downward motion on the paper indicates increase of west declination, and of horizontal and vertical force, and in earth currents corresponds to the passage of a current similar to that from the copper pole of a battery in the direction Angerstein Wharf—Lady Well in the one case, and in the

direction Blackheath—North Kent East Junction in the other. The azimuth of the former line, reckoning from magnetic north to east, is 50°, and of the latter, reckoning from magnetic north to west, is 46°. The direction of strongest earth current at Greenwich is not much different from that of the line joining the A. W.—L. W. earth plates. The current in the A. W.—L. W. circuit is always much stronger than that in the B.—N. K. E. J. circuit, and a current that causes the A. W.—L. W. trace to move in one direction on the paper causes the B.—N. K. E. J. trace to move in the opposite direction.

these sudden movements, which, although abrupt in character, are sometimes of no great magnitude. There is not the same simultaneity of movement in the different elements when disturbance commences, as it were, gradually, or in general during the course of a magnetic storm; the really simultaneous movements are exceptional.

The annexed Table I contains the times of commencement of movement on the seventeen selected days at each of the eight stations. The second column gives the year, day, and hour of disturbance, Greenwich civil time (counting from midnight to midnight), and in following columns are added the times to tenths of a minute for the eight stations, arranged in order of longitude, reckoning from west to east, and reduced to Greenwich time by the application of the differences of longitude given in Table II. From accidental causes, as before mentioned, times were wanting in six cases, and in addition for two in which there were unresolved discordances. These were at Batavia, on 1882, April 16; at Pawlowsk and Mauritius, on April 20; at Melbourne, on August 4; at Toronto, on September 12; at Batavia, on 1883, February 24; at Zi-ka-wei, on September 16; and at Toronto on 1884, July 2. To maintain a proper balance in the table, times in these cases have been adopted by estimation, paying proper regard to the average deviation of the times at each particular station from the general average of all the stations.

The times as received were given usually to the nearest minute; the fractions of a minute that appear in the table are partly due to the time being in most cases the means of those for three magnetic elements, and partly to the fractional value of the longitude used. In a following column is given for each day the mean of the times at the eight stations, and in succeeding columns the deviation of the time at each station from this daily mean.

Every instance of magnetic motion included in the table was accompanied at Greenwich by active earth current. It is known that in some cases earth currents were similarly active at other places, and presumably they were active at all places.

From an inspection of the table it will be seen how nearly these sudden and characteristic magnetic impulses occur at the same absolute time at places geographically widely separated. At some

Table I.—Time of commencement of Magnetic Movement at different Places compared.

No. for reference.	Observed time of commencement of magnetic movement at the different places reduced to Greenwich civil time.									Daily mean of times.	Deviation of time at each place from the mean.							
	Day and hour.	Toronto.	Greenwich.	Pawlowsk.	Mauritius.	Bombay.	Batavia.	Zi-ka-wei.	Melbourne.		Toronto.	Greenwich.	Pawlowsk.	Mauritius.	Bombay.	Batavia.	Zi-ka-wei.	Melbourne.
	d. h.	m.	m.	m.	m.	m.	m.	m.	m.	m.	m.	m.	m.	m.	m.	m.	m.	m.
1	1882. April 16 23 +	33·6	34·0	33·1	35·8	35·0	37·0	31·3	38·4	34·8	−1·2	−0·8	−1·7	+1·0	+0·2	+2·2	−3·5	+3·6
2	20 3 +	32·6	35·7	35·0	36·0	36·7	39·7	32·3	33·4	35·2	−2·6	+0·5	−0·2	+0·8	+1·5	+4·5	−2·9	−1·8
3	June 15 3 +	1·6	4·7	3·1	3·8	3·7	8·7	0·3	5·1	3·9	−2·3	+0·8	−0·8	−0·1	−0·2	+4·8	−3·6	+1·2
4	Aug. 4 15 +	45·6	48·7	48·2	49·8	49·4	48·7	45·3	50·0	48·2	−2·6	+0·5	0·0	+1·6	+1·2	+0·5	−2·9	+1·8
5	Sept. 12 2 +	50·0	53·5	49·2	54·8	50·7	52·7	46·0	55·1	51·5	−1·5	+2·0	−2·3	+3·3	−0·8	+1·2	−5·5	+3·6
6	Oct. 2 9 +	39·6	39·5	39·2	39·8	39·7	42·7	35·6	45·1	40·2	−0·6	−0·7	−1·0	−0·4	−0·5	+2·5	−4·6	+4·9
7	Nov. 17 10 +	17·6	20·0	20·2	23·1	22·0	20·7	21·3	25·1	21·2	−3·6	−1·2	−1·0	+1·9	+0·8	−0·5	+0·1	+3·9
8	1883. Feb. 24 13 +	40·6	41·7	42·2	41·8	40·7	44·0	38·3	45·1	41·8	−1·2	−0·1	+0·4	0·0	−1·1	+2·2	−3·5	+3·3
9	April 3 9 +	0·6	1·0	1·2	−0·2	1·4	4·7	−1·2	0·1	0·9	−0·3	+0·1	+0·3	−1·1	+0·5	+3·8	−2·1	−0·8
10	July 29 23 +	45·6	46·5	47·2	46·1	44·7	48·7	41·8	55·1	47·0	−1·4	−0·5	+0·2	−0·9	−2·3	+1·7	−5·2	+8·1
11	Sept. 16 2 +	43·6	42·0	44·1	44·8	44·0	46·7	41·0	45·1	43·9	−0·3	−1·9	+0·2	+0·9	+0·1	+2·8	−2·9	+1·2
12	1884. July 2 19 +	14·5	15·3	16·1	16·8	16·4	18·7	15·3	15·1	16·0	−1·5	−0·7	+0·1	+0·8	+0·4	+2·7	−0·7	−0·9
13	Oct. 1 21 +	48·6	50·3	52·1	50·5	51·4	54·7	50·3	50·1	51·0	−2·4	−0·7	+1·1	−0·5	+0·4	+3·7	−0·7	−0·9
14	1885. June 24 22 +	30·6	31·0	31·1	31·8	30·4	32·7	27·3	35·1	31·3	−0·7	−0·3	−0·2	+0·5	−0·9	+1·4	−4·0	+3·8
15	1886. Mar. 30 8 +	21·6	22·7	20·1	22·5	21·7	24·7	20·8	25·1	22·4	−0·8	+0·3	−2·3	+0·1	−0·7	+2·3	−1·6	+2·7
16	1888. Oct. 30 19 +	43·6	44·7	48·1	43·8	45·0	45·7	41·3	45·1	44·7	−1·1	0·0	+3·4	−0·9	+0·3	+1·0	−3·4	+0·4
17	1889. July 17 4 +	47·6	50·0	50·1	49·8	49·0	52·7	47·4	45·1	49·0	−1·4	+1·0	+1·1	+0·8	0·0	+3·7	−1·6	−3·9

stations the times are distinctly greater and at others distinctly less than the average of times for all stations. Either the magnetic impulse is really retarded at some stations and accelerated at others, as referred to the mean, or there exists at individual stations some systematic error, mechanical or otherwise, which affects the times in one particular way. That such error should exist is not unlikely since it is difficult to make accurate, beyond a certain point, a time scale so contracted, and difficult also to measure correctly the photographic trace, besides which the desirability of securing such extreme accuracy has not been before made apparent.

It happens that on five of the selected days, Nos. 6, 7, 8, 9, and 10, corresponding records were found in the magnetic section of the 'Mission Scientifique du Cap Horn,' 1882—1883. Here No. 6 is said to be "une diminution brusque," No. 7 "un mouvement très rapide," and No. 9 is said to commence "brusquement." Engraved copies of the registers on a small scale are given for Nos. 8, 9, and 10, showing the movements to have been of the character described. This affords five comparisons. The times are given only to the nearest five minutes, and the mean of the five resulting deviations, which range from +7·1 m. to −8·6 m., is +1·1 m. There were in addition six other instances of sudden movement contained in the Cape Horn series, not included in our selected list, for which corresponding movements were readily found in the Greenwich records. These movements are spoken of as having at Cape Horn the same abrupt character as those already mentioned. The separate values of deviation referred (by comparison through Greenwich) to the general mean of Table I range from +9·2 m. to −10·8 m., the mean being −2·1 m. Or, combining the two sets, we have from eleven comparisons a mean deviation of −0·7 m.

The mean deviation of time at each station from the general mean for all stations (the means of the columns of differences in Table I) and the difference between the greatest and least values at each station, adding the result for Cape Horn for eleven days just found, are as follows:—

Table II.—Mean Deviation of Time at each Station from the General Mean for all Stations, and Difference between the greatest and least Values of Deviation at each Station.

Station.	Latitude.	Longitude.	Mean deviation.	Difference between the greatest and least values.
	° ′	h. m.	m.	m.
Toronto	43 40 N.	5 17·6 W.	−1·5	3·3
Cape Horn..........	55 31 S.	4 32·3 „	−0·7	20·0
Greenwich	51 29 N.	0 0·0	−0·1	3·9
Pawlowsk...........	59 41 „	2 1·9 E.	−0·2	5·7
Mauritius...........	20 6 S.	3 50·2 „	+0·5	4·4
Bombay	18 54 N.	4 51·3 „	−0·1	3·8
Batavia	6 11 S.	7 7·3 „	+2·4	5·3
Zi-ka-wei	31 12 N.	8 5·7 „	−2·9	5·6
Melbourne	37 50 S.	9 39·9 „	+1·8	12·0

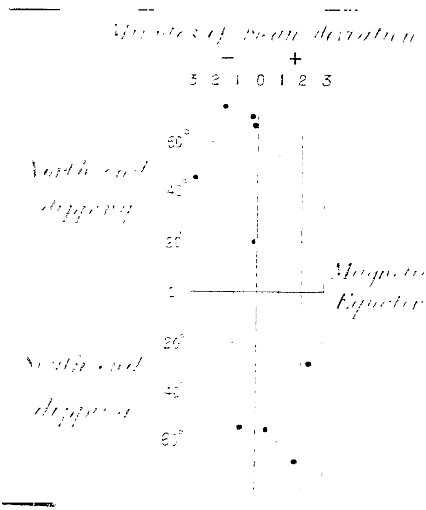
At Cape Horn and Melbourne the times were given only to the nearest 5 minutes; at the other stations they were given to the nearest minute. This may explain the greater differences for Cape Horn and Melbourne in the last column of the table.

The question that now arises is, What is the explanation of the small differences between the values of deviation at different places? As already suggested, they must be due either to real difference in the time of the magnetic impulse, depending on geographical position, or to the existence at individual stations of some systematic error.

In regard to the possible variation of the values of deviation with geographical position, there may be variation with latitude or with longitude. It will be observed that in Table II the stations are already arranged in order of longitude, and the run of the numbers would appear to indicate a tendency to negative values at westerly stations, and to positive values, on the whole, at easterly stations, but yet the differences are such (remembering how easily some of these differences might arise, with existing apparatus) as would hardly warrant the assumption that such variation really exists; neither does a change with longitude seem to be one for which there would appear to be any ready physical explanation. The variation in the values of deviation with latitude is better seen by arranging the numbers of Table II in order of latitude, and, considering the divergence of the magnetic equator from the astronomical equator, it seems better to group them according to the values of magnetic dip at the various stations, which has been done as follows, adding also a

graphical representation of the relation between the values of deviation and dip.

Station.	Value of dip.	Mean deviation.
		m.
Toronto	75° N.	−1·5
Pawlowsk	71 „	−0·2
Greenwich	67 „	−0·1
Zi-ka-wei	46 „	−2·9
Bombay	20 „	−0·1
Batavia	28 S.	+2·4
Cape Horn	53 „	−0·7
Mauritius	55 „	+0·5
Melbourne	67 „	+1·8



There would seem to be here a tendency to negative values with northerly dip, and to positive values with southerly dip. Remarking how five stations (two with extreme northerly dip, two with extreme southerly dip, and one intermediate) hang together, this apparent tendency may be accidental, and yet there is an undoubted drift to the left in the upper part of the diagram, and to the right in the lower part A variation with latitude certainly appears to be one better admitting of physical explanation than does a variation with longitude. Can anything depend on the distribution of land and water? Whether the observed deviations represent a real inequality,

or whether the irregularities are wholly accidental, and that the magnetic impulse in these motions is everywhere simultaneous, we cannot say. The data are not sufficient to permit of the formation of any definite conclusion.

But whatever may be the explanation of the irregularities of deviation, it would seem that over a certain portion of the earth's surface the magnetic action in these initial impulses is practically simultaneous. The values of mean deviation for Greenwich, Pawlowsk, Mauritius, and Bombay are −0·1 m., −0·2 m., +0·5 m., and −0·1 m., respectively, an agreement which is certainly striking, and which can hardly be explained on any supposition but that of simultaneous action. For if we divide the series for these places (Table I) into two groups, say of 8 and 9 values each respectively, and take their means, we have as follows:—

	Greenwich.	Pawlowsk.	Mauritius.	Bombay.	Extreme difference.	Mean.
	m.	m.	m.	m.	m.	m.
Mean of first 8 values...	+0·1	−0·8	+1·0	+0·1	1·8	+0·1
Mean of last 9 values...	−0·3	+0·4	0·0	−0 2	0·7	0·0
Difference	0·4	1·2	1·0	0·3	—	—

In the first set the Greenwich and Bombay values agree, so also in the second set (within 0·1 m.). Greenwich and Bombay are thus in close accord, and in a remarkable degree, the agreement amounting almost to a confirmation of the adopted difference of longitude. The extreme difference between the four values at the different stations is seen to be, in the first set 1·8 m., and in the second set 0·7 m. And the differences between the separate values at each station are severally 0·4 m., 1·2 m., 1·0 m., and 0·3 m. Now the latter differences, which may be taken to indicate the amount of accidental irregularity in the values, bear a considerable proportion to the differences between the values at the separate stations (1·8 m. and 0·7 m.), so that these differences are probably wholly accidental. Again, the mean of the first four values is +0·1 m., and of the last four 0·0 m., showing on the whole no real difference. All this indicates that at Greenwich, Pawlowsk, Mauritius, and Bombay the magnetic impulse would appear to be really simultaneous, or at any rate that the variations between the observed deviations at these places afford no trustworthy evidence against such conclusion.

We come thus to an interesting result. The magnetic impulse over that portion of the earth's surface covered by the places last mentioned would evidently appear to be, on the whole, simultaneous; although whether, in like manner, simultaneous over the whole earth

we cannot say. But, considering the inadequacy of our present registering apparatus to ensure in all cases the necessary accuracy of time indication, it becomes a question whether the coincidence may not be generally closer than present means will enable us to measure. To determine this with precision would, however, require some modification of the apparatus now in general use.

The circumstance that the sudden magnetic movements of the character here discussed appear to be so nearly, if not absolutely, simultaneous over the whole earth becomes, if established, a striking physical fact. In numerous cases the disturbance, commencing suddenly, and continuing for many hours, follows on a state of comparative magnetic calm. How is the sudden change to a state of activity brought about, and what is the influence that so instantaneously disturbs the magnetic condition of our globe? The magnetic movements at Greenwich at such times are always accompanied by corresponding earth currents, which, similarly following a state of calm, become at once active, precisely at the commencement of the magnetic disturbance. It is believed that a similar effect is produced at other places, and that this is so is, in some cases, specially mentioned. Some sudden action, apparently from without, sets up, as it seems, both earth current and magnetic activity simultaneously, or nearly so, over the whole earth. It is to be noted that magnetic and earth current disturbance at Greenwich is, on the whole, more frequent in spring and autumn, when, in its diurnal revolution, the whole surface, or nearly the whole surface, of the earth becomes, during each twenty-four hours, exposed to the sun, as though the earth in some way contributed to the production of the phenomena observed, a given external influence appearing to cause at these times a greater activity than at other parts of the year. For magnetic disturbance rises and falls generally with the existence of more or fewer sun spots, in which variation we do not expect to find an annual period. Without, however, discussing further the production of, or relation between, magnetic disturbances and earth currents, on which something will be said in a later part of this paper, we will proceed to offer some particulars more nearly concerning what has gone before.

In asking for corresponding times of magnetic movement at the places mentioned in Table I, request was also in each case made for information on the direction and character of the magnetic movement, and many interesting particulars were thus supplied. It is not proposed on the present occasion to treat these details numerically, but rather to group them in such a way as to show, in a simple and ready manner, the direction of the magnetic movements at each place on the several days. The results are given in Table III, in which the sign + indicates that the north end of the declina-

tion magnet was drawn towards the west, the north end of the horizontal force magnet towards the north, and the north end of the vertical force magnet downwards; the sign – indicating in each case the opposite movement. At Mauritius, Batavia, and Melbourne, at which places the south end of the needle dips, the sign + indicates that the south end of the vertical force magnet was drawn downwards. In the absence of definite information, this is presumed to apply also to Cape Horn, at which the south end also dips. The movement in horizontal force appears to have been in general the most significant. At the stations in tropical regions the movements in declination and vertical force were usually small.

The table gives an interesting synopsis of the direction of magnetic movement in the several cases, and the general similarity of movement at each station on different days. Thus the movements in the three magnetic elements on different days at Cape Horn, Bombay, Batavia, and Zi-ka-wei were, so far as information is available, in each case similar throughout, and, with one or two exceptions, also at Greenwich, Pawlowsk, and Mauritius; at Melbourne there were variations, and at Toronto, for which place information in regard to the declination change only was received, there were also variations. At Greenwich the increase in all elements was always accompanied by earth current of one character as respects direction of current, the instances of decrease in the magnetic elements being accompanied by earth current of opposite character.

The general, indeed remarkable, similarity in the character of the initial magnetic impulse on different days at the several places, considered in connexion with the coincidence in time, indicates the advent in these cases of some powerful disturbance or sudden shock influencing the magnetism of the whole earth nearly always in the same way, an influence in which the earth becomes, as it were, at once involved, a state of general magnetic calm being at all places immediately converted into a state of magnetic activity. Whether these magnetic movements occur more frequently when any particular region of the earth is turned towards the sun is a question that the selected cases of Table I, with the additional instances to be found in Table IV, are too few to determine; but this point may receive elucidation from a more extended inquiry which the many years of Greenwich records will readily afford, and with which object some tabulation thereof has been already made.

Our inquiry has been confined to a consideration of the initial magnetic movements preceding disturbance. As pointed out, these movements occur simultaneously, both for the different magnetic elements and for all places. The character of the magnetic change is mostly similar at any one place on different days, but is not necessarily the same for all places; that is to say, the initial phenomena become, on

the different occasions, repeated at each station in a generally similar manner, but with the individual difference peculiar to the station. Disturbance having, however, once set in, it is found that, although the magnetic irregularity will be practically similar for places not geographically distant, as Greenwich and Kew, there is not the same complete similarity in the movements at places widely separated. The magnetic impulse at any moment during disturbance may, of course, be simultaneously propagated through the earth, as in the case of the initial movements; but it is not usually possible to fix on any phase of movement, and say to what phase it corresponds at another distant station, unless the movement is sudden, when the initial type again recurs, and such recurrence, occurring during disturbance at Greenwich, has been found to correspond with recurrence at other places. Generally it is only these cases of sudden, not necessarily large, movement, occurring at the beginning or during a magnetic disturbance, that we can readily identify as being really simultaneous and corresponding movements.

Reference has been made to the earth currents always so active during magnetic disturbance. Mr. C. V. Walker, formerly telegraph superintendent of the South Eastern Railway, in a paper read before the Royal Society in the year 1861, came to the conclusion, from observations made on the telegraph lines under his control, that earth currents, at least at times of great magnetic disturbance, exercised a direct action upon magnetomoters, just as artificial currents confined to a wire exercise a direct action upon a magnet. Two insulated lines of wire were afterwards established in connexion with the Royal Observatory, one passing from the Observatory to Dartford, the other from the Observatory to Croydon, expressly for the study of earth currents. The distance between the earth plates in the Dartford circuit was 10 miles, and in the Croydon circuit 8 miles. Sir George Airy, discussing the earth currents observed in these lines during the years 1865 to 1867 ('Phil. Trans.,' vol. 158, p. 471), confirmed generally Mr. Walker's result, considering that "it is impossible to avoid the conclusion that the magnetic disturbances are produced by terrestrial galvanic currents below the magnets;" but there were some anomalies, the earth current appearing, in some cases, distinctly to follow the magnetic motion, instead of being coincident with, or preceding, it, as, on the supposition mentioned, should always happen, and in other cases preceding it by a longer interval of time than the conditions seemed to require.

In the year 1868 the earth current lines to Dartford and Croydon were replaced by two others running, one from near Morden College, Blackheath, to the North Kent East Junction of the South Eastern Railway (distance between earth plates 2½ miles), and the other from Angerstein Wharf (on the bank of the River Thames, near Charlton)

Table III.—Observed Direction of First Movement for the Magnetic Elements of Declination, Horizontal Force, and Vertical Force for the 17 days given in Table I, and for the 5 days on which there are corresponding records for Cape Horn. (It was remarked on all occasions at Toronto, and on various occasions at Greenwich and Pawlowsk, that there was a preceding movement, usually small, in the direction opposite to the direction given in the table as that of first movement.)

No. for reference.	Civil day.	Magnetic element.	Name of place, and direction of magnetic movement.								
			Toronto.	Cape Horn.	Greenwich.	Pawlowsk.	Mauritius.	Bombay.	Batavia.	Zi-ka-wei.	Melbourne.
1	1882. April 16....	dec.	—	..	+	+	—	+	..	—	+
		h.f.	..	..	+	+	+	+	..	+	—
		v.f.	..	..	+	+	..	—	..	..	+
2	April 20....	dec.	—	..	+	..	..	+	+	—	+
		h.f.	..	..	+	..	..	+	+	+	—
		v.f.	..	..	+	..	..	—	—	..	+
3	June 15....	dec.	—	..	+	+	+	+	+	—	+
		h.f.	..	..	+	+	+	+	+	..	+
		v.f.	..	..	+	+	..	—	—	..	—
4	Aug. 4....	dec.	—	..	+	..	—	+	+	..	..
		h.f.	..	..	+	..	+	+	+	+	..
		v.f.	..	..	+	..	+	—	—	..	..
5	Sept. 12....	dec.	..	..	+	+	—	+	+	—	+
		h.f.	..	..	+	+	+	+	+	+	+
		v.f.	..	..	..	+	+	—	—	—	—
6	Oct. 2....	dec.	+	+	+	+	—	+	+	—	+
		h.f.	..	+	+	±	+	+	+	+	+
		v.f.	..	—	..	..	+	—	—	—	—
7	Nov. 17....	dec.	+	..	—	±	—	+	+	—	—
		h.f.	..	..	—	+	..	+	+	+	+
		v.f.	..	..	—	±	..	—	—	—	—
8	1883. Feb. 24....	dec.	—	+	+	+	..	+	..	—	..
		h.f.	..	..	+	+	+	+	..	+	+
		v.f.	..	—	+	+	+	—	..	..	—
9	April 3....	dec.	+	+	—	—	+	+	+	..	—
		h.f.	..	+	—	+	+	+	+	+	+
		v.f.	..	..	—	—	+	—	—	—	—
10	July 29....	dec.	—	+	+	+	—	+	+	—	+
		h.f.	..	+	+	+	+	+	+	+	—
		v.f.	..	—	..	..	+	—	—	..	—

Table III—*continued.*

No. for reference.	Civil day.	Magnetic element.	Toronto.	Cape Horn.	Greenwich.	Pawlowsk.	Mauritius.	Bombay.	Batavia.	Zi-ka-wei.	Melbourne.
			Name of place and direction of magnetic movement.								
11	1883. Sept. 16....	dec.	–	..	+	+	–	+	+	..	–
		h.f.	..	..	+	+	+	+	+	..	+
		v.f.	..	..	+	–	+	–	–	..	..
12	1884. July 2....	dec.	..	..	+	+	±	+	+	..	–
		h.f.	..	..	+	+	+	+	+	+	+
		v.f.	..	..	+	+	+	–	–	–	..
13	Oct. 1....	dec.	+	..	+	+	–	+	+	..	–
		h.f.	..	..	+	+	+	+	+	+	+
		v.f.	..	..	+	+	+	–	–	..	..
14	1885. June 24....	dec.	–	..	+	+	–	+	+	–	..
		h.f.	..	..	+	+	+	+	+	+	+
		v.f.	..	..	+	..	+	–	–	..	..
15	1886. Mar. 30	dec.	+	..	±	..	±	+	+	..	+
		h.f.	..	..	±	..	+	+	+	..	+
		v.f.	..	..	±	..	+	–	–	..	–
16	1888. Oct. 30....	dec.	–	..	+	+	..	+	+	..	..
		h.f.	..	..	+	+	..	+	+	+	+
		v.f.	..	..	+	..	+	–	–	..	..
17	1889. July 17....	dec.	–	..	+	+	+	+	+	–	+
		h.f.	..	..	+	..	+	+	+	+	+
		v.f.	..	..	+	..	+	–	–	–	..

± indicates that the direction of first motion was doubtful.

.. indicates that no information was received, or that there was no record, or (in a few cases) that the change was small.

to Lady Well, Lewisham (distance between earth plates 3 miles): these positions being selected in order that the recording apparatus at the Royal Observatory, through which the wires passed, should be nearly in the middle of the straight lines joining the respective earth plates. Now it has been mentioned that the instances of sharply-defined magnetic impulses contained in Table I were all accompanied (in the just-described newer earth current lines) by active earth currents, in themselves just as sharply defined. The distinctive character and generally isolated position of these magnetic and earth current movements thus afford a peculiarly favourable opportunity for investigating any relation that may exist between them, much more so than

would any attempt to discuss movements of a mixed or involved nature, and it is found that motions of the declination and horizontal force magnets for the instances contained in Table I are, without exception, in direction such as would be produced by the action of a current passing through a wire under the magnet, similar to that of the actual accompanying earth current.

But then comes a further question:—Do earth current and magnetic times really agree? For in the discussion of the Dartford and Croydon earth current records, the earth current on some occasions, according to the instrumental time scales, seemed undoubtedly to follow the magnetic movement, instead of being coincident with, or preceding, it. But the system on which the time scales were in those days laid down was not arranged with a view to the accurate measurement of small intervals of time, and in this respect did not permit of an accuracy equal to that to be obtained from later registers, on which a time indication is made at every individual hour. Thus we are now able to effect a much more certain comparison of the times of magnetic and earth current movement than was before possible. This has been done for a considerable number of instances of sudden initial movement, including all those of Table I (excepting 1882, April 16, on which day the earth current register was defective), and also a number of others, the whole particulars being given in Table IV.

When it is considered that each separate difference in this table includes the sum of the errors of two time measures from photographs having a time scale in which one minute corresponds to about 0·01 inch, the agreement of results, it will be seen, is very good. Every case selected for measurement is given; nothing has been rejected. The variation between individual differences does not exceed in amount that which may well be expected, with a time scale so contracted, and would appear to be simply due to unavoidable errors of measurement. The mean of the 31 differences is −0·14 m., equivalent to 8 sec., or, dividing the 31 differences into three groups of 11, 10, and 10 values respectively, the means of these groups are found to be −0·19 m., −0·12 m., and −0·10 m., equivalent to 11 sec., 7 sec., and 6 sec. respectively, thus persistently showing the earth current to precede by a few seconds the magnetic movement. Not that these results should be taken too strictly, but simply as showing, when definite comparison can be made, that the coincidence of time is really very close, possibly more so than present existing means will permit us to determine. This so far removes the before-mentioned time anomaly, the observed facts being now seen to be altogether consistent with the supposition that earth currents may produce the magnetic movements, whether or not, as regards cause and effect, this be the true relation. For it has to be remembered that in

Table IV.—Comparison of the time of commencement of Magnetic Movement with the appearance Active Earth Current, preceding Magnetic Disturbance, at Greenwich.

Greenwich civil time of commencement of magnetic movement.		d.	h.	m.	Minutes of corresponding earth current.	Excess of the latter.	Greenwich civil time of commencement of magnetic movement.		d.	h.	m.	Minutes of correspond earth curr	Excess of the latter.
					m.	m.						m.	m.
1880.	Aug.	11	10	18·4	20·0	+1·6	1882.	Nov.	25	16	28·0	28·0	0·0
		12	11	37·7	35·0	−2·7	1883.	Feb.	24	13	41·7	41·0	−0·7
1881.	Jan.	30	19	41·5	40·0	−1·5		April	3	9	1·0	1·0	0·0
		31	8	37·4	38·0	+0·6		June	30	5	14·7	13·0	−1·7
1882.	April	20	3	35·7	37·0	+1·3		July	29	23	46·5	47·0	+0·5
	June	15	3	4·7	4·0	−0·7		Aug.	15	9	50·0	49·0	−1·0
	Aug.	4	5	50·0	50·0	0·0		Sept.	16	2	42·0	43·0	+1·0
		4	15	48·7	48·0	−0·7	1884.	July	2	19	15·3	16·0	+0·7
	Sept.	11	22	41·0	40·0	−1·0		Oct.	1	21	50·3	51·0	+0·7
		12	2	53·5	54·0	+0·5	1885.	June	24	22	31·0	31·0	0·0
	Oct.	2	9	39·5	40·0	+0·5			25	3	41·7	42·0	+0·3
	Nov.	11	23	17·3	17·0	−0·3	1886.	Mar.	30	8	22·7	22·0	−0·7
		15	8	14·7	13·0	−1·7	1888.	Oct.	30	19	44·7	44·0	−0·7
		16	8	17·3	17·0	−0·3	1889.	July	17	4	50·0	49·0	−1·0
		17	10	20·0	23·0	+3·0	1891.	Mar.	2	1	49·3	49·0	−0·3
		19	12	43·0	43·0	0·0							

magnetic disturbances the variations of vertical force, which are sometimes very great, have also to be considered, as well as those of declination and horizontal force. But even admitting the supposition to be correct, there remains for consideration the question as to how the diurnal magnetic variations are produced.

Magnetic disturbances and irregular magnetic motions of every kind are always accompanied at Greenwich by earth currents more or less powerful, the correspondence being most complete, as may be seen in the engraved copies of magnetic and earth current movements given since the year 1882 in the annual Greenwich volume. The diurnal magnetic variation of declination or horizontal force, on the other hand, which (when there is no accidental magnetic irregularity) consists principally of one bold daily sweep, has no corresponding earth current, being accompanied principally by alternating currents, weak in character, and generally of short period. That is to say, a sudden magnetic movement of an amplitude no greater than that of the undisturbed diurnal curve (comparing one part of the day with another) will be accompanied by active earth current, whilst the bold sweep of the undisturbed diurnal curve has no marked earth current counterpart, but only fluctuations of feeble character. The amplitude of the diurnal magnetic variation may be greater than the motion in many of the cases of movement included in Table I, but the earth current in the former case would be insignificant, whilst in the latter it is most marked. Irregularity in magnetic action and activity of earth current are correlative phenomena, rising and falling together in intensity, earth current activity indicating irregularity of magnetic movement or magnetic storm, superposed on, and, indeed, frequently masking, the ordinary diurnal curve.

The assumption that the magnetic movements in a magnetic storm are due to action of the accompanying earth currents thus appears not to be one that will explain the ordinary diurnal magnetic variation, as, indeed, Sir George Airy had previously found ('Phil. Trans.,' vol. 160, p. 226) from discussion of the earth currents observed in the old Dartford and Croydon earth current lines. The phenomena have, indeed, different characteristics. The period of the diurnal magnetic variation is the solar day, and the principal sweep in the curve occurs whilst the sun is above the horizon. Magnetic irregularities, on the other hand, and their accompanying earth currents, appear at any moment. Although both probably solar in origin, they are not produced in the same way. The diurnal variation progresses gradually, the principal phase occurring successively at different places, as they become turned towards the sun, but magnetic irregularities arise suddenly and simultaneously at all places. The special characteristics of the two classes of phenomena may be stated as follows:—

	Diurnal magnetic variation.	Superposed magnetic disturbance.
Characteristic.	One bold sweep progressing uniformly.	Irregular motions.
Occurs at different places.	Successively.	Simultaneously.
Earth current condition.	No corresponding earth current; only weak intermittent currents.	Active earth currents.

However bold the diurnal variation may be, so long as it progresses uniformly, the earth currents remain fluctuating and feeble, and apparently cannot produce the long sweep of the diurnal curve. But if any magnetic irregularity arises, a corresponding earth current at once appears. It was pointed out with respect to the magnetic movements on the selected days (Tables I and III), that the earth currents observed at Greenwich in connexion therewith always had a definite relation with the motions in declination and horizontal force. This, taken in connexion with the coincidence in time (Table IV), certainly suggests mutual relation. But, in view of the circumstance that the diurnal magnetic variation does not appear to depend on earth current, can we suppose that it may be otherwise with the irregular magnetic motions, and that they are produced by earth currents? When a magnetic storm arises the earth would appear to become as a whole instantaneously affected, necessarily, it would seem, from without. Is it that both classes of phenomena, irregular magnetic variations and earth currents, are produced by independent action, and that any mutual relation, if existing, is of secondary character. In the Greenwich registers the variations of horizontal force, during periods of magnetic disturbance, follow with surprising closeness the accompanying earth current variations, the turning points being in a very remarkable degree simultaneous. But, after the initial movement, there is not a similar resemblance as regards declination changes. And how are the variations of vertical force produced? Can there be here any relation with earth currents?

The general direction of strongest earth current at Greenwich at the present time is one not very different from that of the line joining the Angerstein Wharf—Lady Well earth plates. Does this direction of strongest earth current depend on geological formation, or is it slowly variable, as are the magnetic elements? If not variable, the earth current influence on the declination and horizontal force magnets, if such influence exists, would be different when the direction of the magnetic meridian has greatly changed. This is a point the study of which might in the course of time throw considerable light on the relation between magnetic and earth current irregulari-

ties, in regard to which there is yet much to learn. It would tend to help this, as well as other inquiries concerning earth current phenomena, if at some of the magnetic observatories in different parts of the world, the magnetic registers were supplemented by a registration of earth currents, as a regular part of the daily work.

The comparison of the times of magnetic impulses with those of the corresponding earth currents, Table IV, gave, as a mean result, that, at Greenwich, the magnetic impulse, on the whole, followed the earth current by some few seconds. To determine more exactly and with greater certainty, and especially in individual cases, what may really be the interval, if appreciable, between the phenomena would require apparatus designed to indicate simultaneously and measure accurately much smaller intervals of time than is at present possible. This leads us to mention a difficulty that affects the registration of magnetical and meteorological phenomena of all kinds, and one not readily overcome. To obtain continuous registration of any element a contracted time scale is necessary, otherwise the accumulated registers would become overwhelming, neither is an extended time scale, under ordinary circumstances, required. But on occasions of disturbance or storm, whether magnetic or atmospheric, it becomes desirable to possess the power of greatly increasing the extent of the time scale, in order to obtain, not only a more accurate time indication, but also a better record of the details of phenomena. The difficulty becomes increased because of the uncertainty of the advent of any special phenomenon of which fuller particulars would be desirable. Still, if it were possible to provide means of easily changing the speed of movement of a register, much valuable information might be gained.

In recapitulation of the results which may be considered to have been arrived at in this paper, we may remark that, though it was known that magnetic storms were felt generally at the same time over wide areas of the earth's surface, it had not been ascertained that any magnetic movements were so entirely coincident at different places as now appears, at any rate so far as concerns the initial magnetic impulses that precede disturbance, and which, it would seem, really occur at the same instant of time, or nearly so, over the whole earth. It appears also, in addition, that the change that takes place at such times in the earth's magnetic condition is, on all occasions, in great measure of like character. A definite magnetic effect is produced suddenly and simultaneously, in which the variations of the magnetic elements, whilst different at different places, are, on different occasions, locally similar, forming thus a type of the magnetic phenomena that, repeating itself usually in the same way, generally presages or introduces a magnetic disturbance or storm.

It has been further shown that at Greenwich the sudden magnetic impulses immediately preceding or commencing disturbance are concurrent in time with the sudden appearance in each case of an earth current, in advance by a few seconds of the magnetic impulse, and having always the same relation with the magnetic movements, increase of magnetic declination, horizontal force, and vertical force, being accompanied by a current in one and the same direction, and decrease of these elements by a current in the opposite direction. A like concurrence in time between such magnetic movements and earth currents is presumably true also for other places.

If the near time relation thus established between initial magnetic and earth current movements at Greenwich applies generally during the course of a magnetic storm, any difficulty as respects the assumption that the irregular changes of magnetic declination and horizontal force may be produced by the accompanying earth currents seems removed. But though the changes of horizontal force during a magnetic disturbance closely follow the earth current changes, those of declination do not show the same correspondence, and the variations of vertical force have also to be explained, in addition to which it would conclusively seem that the diurnal magnetic variation does not depend on earth current, since the bold sweep of this curve (when undisturbed) is accompanied by comparative earth current calm.

With reference to the comparison of times of magnetic impulses at different places, it does not seem probable that with existing apparatus any better result would be obtained by making comparison for an increased number of *days*. But it would be interesting, even with the present apparatus, to obtain corresponding times, if possible, for a greater number of *places*, in order more conclusively to determine whether the constant difference of time that appears to exist between some stations (Table II) is really a physical fact, or whether it may not be due to small systematic error in the individual registering apparatus. Registers on an extended scale, on the system mentioned in a preceding paragraph, would, however, be much more likely to give definite information on this point.

If this paper has added anything to the knowledge of a difficult subject, it will be felt that some labour has not been expended in vain.

"Contribution to the History of the Interchange of Pulmonary Gases in the Respiration of Man." By WILLIAM MARCET, M.D., F.R.S. Received June 9,—Read June 16, 1892.*

I had the honour of communicating a paper to the Royal Society in June, 1891, embodying the results obtained from a first series of inquiries on the interchange of pulmonary gases. The investigation has been continued, and I have had the benefit of the assistance of Mr. G. P. Darnell Smith, B.Sc., in the present work. I am greatly indebted to Mr. Smith for many useful suggestions; while our discussions have been fruitful on many occasions.

The apparatus used for the estimation of carbonic acid and oxygen has been fully described in my former paper, but, as the drawing of the eudiometer employed could not be inserted in that communication, it is appended to this paper together with an explanation of the construction of the instrument.

The key to a method of investigation calculated to give correct information on the interchange of the respiratory gases is to be found in the means adopted for the determination of the volumes of air inspired and expired. C. Speck in his experiments, inspired a measured volume of air from a bell-jar, carefully balanced over a water trough, and expired it into another, the volumes of air inspired and expired being thus determined by direct measurement. Messrs. Hanriod and Richet used gas meters instead of bell-jars. In my former inquiry, after trying how closely experiments with bell-jars agreed with each other, it was found that the results obtained were not sufficiently reliable to be used except in the form of means; on that account, the object was attained by calculation, assuming that the volume of nitrogen in the air expired was exactly the same as the volume of nitrogen in the air inspired; consequently the volume of nitrogen expired was proportional to the volume of air inspired.

It has occurred to me that an objection to this means of determining the volume of air inspired might be raised on the ground that it is unscientific to adopt a method of inquiry based on an assumption, although the investigations of Regnault and Reiset have apparently placed what I call an assumption on the footing of an acknowledged fact. There was consequently a gap to fill up, as it was necessary to ascertain whether nitrogen is either absorbed in the blood or given out, or whether it takes no appreciable part in the respiratory process; with this object, an experiment was made by re-breathing a known volume of air in a bell-jar, and determining afterwards the volume of nitrogen present in this air, but the

* Revised August, 1892.

method proved unsatisfactory. Another experiment was made with three bell-jars—a small one, of a capacity of 10 litres, being placed between two others which held, when full, 40 litres each; the three bell-jars were connected with each other by india-rubber tubing in the course of which an apparatus was disposed for the absorption of carbonic acid. The person under experiment breathed out of and into the middle and smaller bell-jar, which held at the time 5 or 6 litres of air, while a known volume of air was driven from one of the large bell-jars into the other, through the smaller receiver and back again. The carbonic acid absorbed was replaced by oxygen gas. The principle of the method was indeed the same as that adopted by Regnault and Reiset, and more recently by Messrs. Jolyet, Bergonié, and Sigalas.* Great difficulty, however, was met with in this experiment, especially from the tension of gases in the bell-jars, and it was ultimately given up.

At last the simplest method was resorted to; although, the results of the experiments varying somewhat widely from each other, this process necessitated the adoption of means.

The experiments were conducted much on the same principle as those made by C. Speck, every precaution being taken to carry them out as correctly as possible. For this purpose, 40 litres of atmospheric air were introduced into one of my bell-jars, the air being saturated by passing it slowly through a glass vessel holding cotton-wool wetted with tepid water. Air was inspired from this bell-jar through the nose, with nose-pieces fitting closely into the nostrils, and expired from the mouth into another bell-jar, also holding 40 litres; corrections were made for slight differences between the bell-jars and for temperatures. In some of the experiments a valve was placed in the tract of the air inspired, and in others no valve was used, but the inspiratory and expiratory india-rubber tubes were pinched alternately with the fingers to prevent any regurgitation. Notwithstanding every care to breathe as naturally as possible, the relations between the volumes of air inspired and expired were occasionally found to differ widely from each other. On this account it has been found necessary to reject a certain number of these experiments, which yielded figures clearly far distant from those they should have given; and I have finally a series of ten experiments to record, in which it will be observed that, although there is a very marked difference between the figures obtained, still the means show that nitrogen takes no appreciable part in the phenomenon of respiration, if concerned at all in this function.

The following are the results obtained, placed in a tabular form—

* References to these papers were given in my former communication.

No. of experiment.	Volumes of air observed.		Volumes of air inspired, obtained by calculation from the volume of nitrogen found in the air expired.
	Inspired.	Expired.	
1 (self)	33,944 c.c.	33,654 c.c.	34,000 c.c.
2 "	34,238 "	33,833 "	33,972 "
3 "	33,686 "	33,445 "	33,783 "
4 "	33,705 "	33,585 "	33,843 "
5 (Mr. Smith) ...	33,996 "	33,840 "	34,262 "
6 " ...	33,945 "	33,774 "	33,953 "
7 (self)	33,226 "	32,886 "	33,106 "
8 "	34,553 "	34,366 "	34,658 "
9 (Mr. Smith) ...	33,194 "	32,926 "	33,101 "
10 " ...	32,912 "	32,585 "	32,754 "
Means	33,740 "	33,489 "	33,743 "

In this table the first column of figures, on the left, shows the volumes of air inspired, as measured in the bell-jar from which the air was breathed; the second column shows the corresponding volumes of air expired as determined in the second receiver. The third column gives the volumes of air inspired, calculated from the assumption that the whole of the nitrogen exhaled is also inhaled.

It will be observed that the mean of the volumes in the third column is all but exactly the same as the mean of the volumes in the first column, the difference being only by 3 c.c.; hence the obvious conclusion that nitrogen takes no appreciable part, or no part at all in the interchange of the pulmonary gases.

It follows from the harmony found to exist between the mean volumes of air actually inspired and the mean volumes of air inspired determined by calculation, that the corresponding volumes of nitrogen, of oxygen consumed, and oxygen absorbed and retained in the blood, whether found or calculated, will also be practically the same; this is set forth clearly in the next table.

These experiments show most satisfactorily that the method adopted to obtain the volumes of air inspired by *calculation* can be trusted as correct; and, therefore, that not only the means obtained by that method, but also every single experiment, may be accepted as giving a reliable result. A certain amount of training was, however, required before the results could be considered as fit for recording.

The next point for consideration refers to the influence of changes of temperature on the consumption of oxygen per minute, or, in other words, on the carbonic acid produced and oxygen absorbed. These experiments have been made on myself and Mr. Smith while under the influence of food, and an additional number were made on

	Nitrogen actually present in air inspired.	Nitrogen by calculation in air inspired.	Oxygen consumed.		Carbonic acid produced.	Oxygen absorbed per minute.		Duration of the experiment.
			Found.	Calculated.		Found.	Calculated.	
W. M.	26,810 c.c.	26,853 c.c.	1775 c.c.	1806 c.c.	1460 c.c.	48 ·3 c.c.	53 ·7 c.c.	6 m. 31 s.
W. M.	27,052 „	26,841	1816 „	1760 „	1622 „	30 ·7 „	21 ·9 „	6 19
W. M.	26,614 „	26,695	1873 „	1895 „	1557 „	45 ·0 „	48 ·2 „	7 1
W. M.	26,634 „	26,741	1857 „	1886 „	1628 „	39 ·1 „	44 ·1 „	7 17
D. S...........	26,864 „	27,074	2240 „	2296 „	1874 „	49 ·6 „	57 ·1 „	7 23
D. S...........	26,823 „	26,830	2104 „	2105 „	1926 „	26 ·4 „	26 ·6 „	6 44
W. M.	26,249 „	26,154	1887 „	1862 „	1642 „	32 ·1 „	28 ·8 „	7 38
W. M.	27,297 „	27,380	2005 „	2027 „	1735 „	38 ·8 „	41 ·9 „	6 58
D. S...........	26,224 „	26,150	1814 „	1768 „	1593 „	36 ·4 „	28 ·8 „	6 5
D. S...........	26,004 „	25,879	1859 „	1826 „	1657 „	33 ·4 „	27 ·9 „	6 3
Means........	26,657 c.c.	26,660 c.c.	1923 c.c.	1923 c.c.	1669 c.c.	37 ·98 c.c.	37 ·90 c.c.	6 48

Mr. Smith while fasting; hence, in his case, the experiments show the influence of food on the interchange of pulmonary gases. I regret that in the present inquiry no experiments were made on myself while fasting, and that those, fasting, on Mr. Smith are too few to show any influence of temperature under this condition. It would have been, of course, more satisfactory to eliminate the influence of food while inquiring into the action of differences of temperature on the interchange of the pulmonary gases. The results obtained show the effects of a change of temperature on the chemical phenomena of respiration to vary somewhat with different persons, some being more disposed than others to react against an accession of cold by an increased production of carbonic acid. In my case, the effect is very obvious, as seen in the following table (p. 218).

First of all, if the means of the oxygen consumed per minute, the carbonic acid produced, and the oxygen absorbed, also per minute, be compared with the corresponding figures obtained under the influence of food in my former inquiry,* they will be found to harmonise with them in a marked degree; they are as follows:—

	In former paper.	Obtained recently.
Oxygen consumed........	248 c.c.	247 c.c.
Carbonic acid produced...	218 „	212 „
Oxygen absorbed	30·2 „	35·7 „

The oxygen consumed is nearly exactly the same in both cases, varying by only 1 c.c. The carbonic acid produced is a little lower in the recent experiments, and the oxygen absorbed somewhat higher.

The slight difference between the carbonic acid produced in each set of experiments, and also the difference, which is rather greater, between the figures found for oxygen absorbed in the two series of experiments, 30·2 and 35·7, are probably owing to the precaution taken in the recent inquiry of allowing half an hour's perfect rest, in the recumbent posture, before commencing the experiment. Formerly the time of rest had been limited to from five minutes to a quarter of an hour, when the pulse and breathing had become perfectly regular; but it was subsequently found that half an hour at least should be allowed before collecting the air expired for analysis, in order to ensure the body being in a perfect state of rest.

The table has been disposed in a graphic form, the curves showing, at a glance, the influence of temperature on the interchange of pulmonary gases in my case; this influence† may be summarised as follows:—

* 'Roy. Soc. Proc.,' June, 1891, vol. 50.

† The influence of cold or heat in the present experiments applies exclusively to

Table showing the Influence of Changes of Temperature on the Carbonic Acid Produced and Oxygen Absorbed per minute. (W. Marcet, aged 64, under experiment.)

Duration of experiment.	Time after full meal.	Temperature of laboratory.	Oxygen consumed.	Carbonic acid produced.	Oxygen absorbed.	Ratio of O. consumed to CO_2 produced.	Ratios grouped two by two.
m. s.	h. m.	° C.	c.c.	c.c.	c.c.		
{ 7 8	2 15	11·4	262	238	24·9	0·909 }	0·916
{ 7 0	2 35	11·4	259	239	20·4	0·923 }	
8 11	1 45	11·7	257	211	46·1	0·821	0·821
7 57	2 15	13·4	243	211	31·8	0·868	0·868
7 22	1 30	13·5	265	228	37·3	0·860	0·860
{ 7 55	2 45	14·4	229	194	35·6	0·844 }	0·826
{ 8 30	2 55	14·4	240	194	46·0	0·808 }	
{ 7 50	2 55	14·4	238	201	37·1	0·844 }	0·831
{ 8 6	3 10	14·4	232	190	42·2	0·819 }	
8 20	2 55	16·3	239	193	45 9	0·808	0·808
Means 7 51	2 30	13·5	246	210	36·7	0·850	

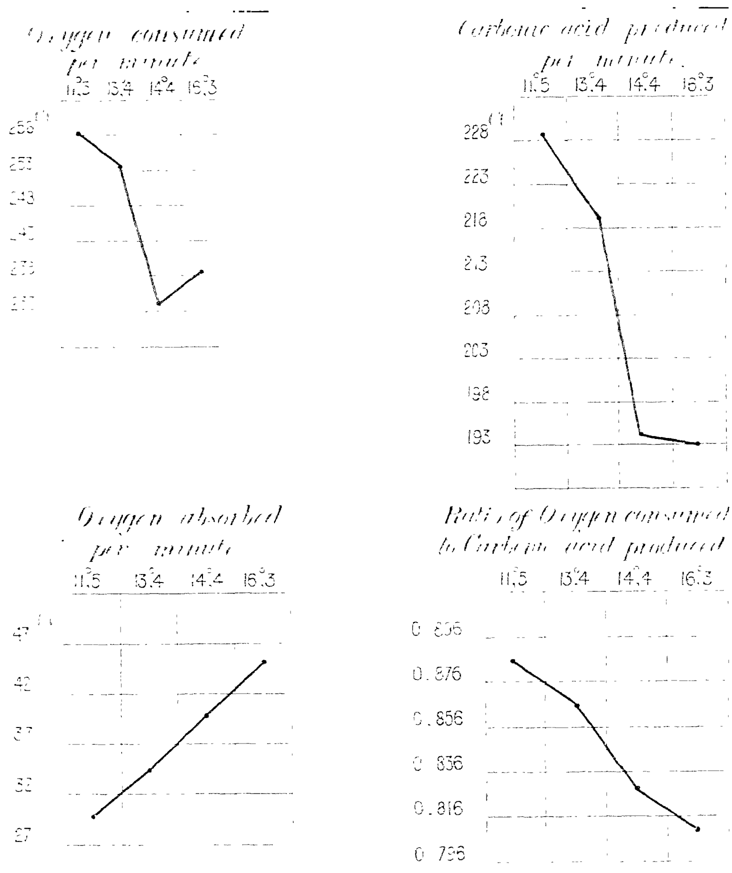
The experiments bracketed have been made at the same sitting.

1st. The oxygen consumed falls as the temperatures rise.

2nd. The carbonic acid produced falls at a similar rate, under the same conditions of temperature, or as the temperatures rise.

3rd. The oxygen absorbed increases as the temperatures rise.

4th. The ratio of oxygen consumed to carbonic acid produced falls as the temperatures increase. This is a natural consequence of the corresponding variation of the oxygen consumed and carbonic acid produced.



This chart is made by grouping the temperatures as follows, and classing the figures for oxygen consumed, carbonic acid produced,

winter temperatures, when an accession of cold is to be met by an increased oxidation in the body. Summer temperatures may act altogether in a different way.

oxygen absorbed, and ratios (under the form of means), together with their respective temperatures:—

	11°·4	13°·4	14°·4	16°·3
	11°·4	13°·5	14°·4	
	11°·7		14°·4	
			14°·4	
Means....	11°·5	13°·4	14°·4	16°·3

Of course it cannot be expected, in a work of this kind, that tracings will be perfectly regular; hence in the tracing for oxygen consumed we observe a slight abnormal rise between 14°·4 and 16°·3. This is probably owing to the weather being cold outside; the experiment was done on the 24th of February; the maximum temperature of that day in the open air was 9° C. (Wimbledon Park), and the body was partly under this cooling influence, although the temperature of the laboratory, at University College, heated with hot-water pipes, was 16°·3. The tracing for carbonic acid produced also exhibits irregularities, but the curves show distinctly that, at all events in my case, changes of temperature exert a very positive influence on the interchange of the pulmonary gases.

If the temperatures of the atmosphere in which the experiments are made be divided into two groups, one group including the lowest and another the highest, and a table constructed, into which the carbonic acid produced and oxygen absorbed are entered together with their corresponding temperatures, this table will be as follows:—

Low temperature.			High temperature.		
Temperature.	CO_2 produced. per min.	O absorbed per min.	Temperature.	CO_2 produced per min.	O absorbed per min.
° C.	c.c.	c.c.	° C.	c.c.	c.c.
11·4	238	24·9	14·4	194	35·6
11·4	239	20·4	14·4	194	46·0
11·7	211	46·1	14·4	201	37·1
13·4	211	31·8	14·4	190	42·2
13·5	228	37·3	16·3	193	45·9
Means 12·3	225	32·1	14·8	194	41·4
Mean ratio of CO_2 produced to O absorbed = 0·143.			Mean ratio of CO_2 produced to O absorbed = 0·213.		

The mean ratio of CO_2 produced to O absorbed is, therefore, very different according to temperatures; it is much lower in relatively cold than in warmer temperatures; hence the interesting fact that the absorption into the blood of that proportion of oxygen consumed which is not transformed into carbonic acid is not concerned in the production of heat towards resisting external cold. This is clearly the case, as the colder the atmosphere the less the proportion of oxygen consumed as *absorbed* in opposition to the proportion consumed towards the production of carbonic acid, which is markedly increased. A similar effect of temperature is met with in the experiments reported in my former paper. Out of six experiments made on myself under the influence of food, two were carried out 2 hrs. 25 mins. and 1 hr. and 40 mins. respectively after a meal, when the laboratory in winter was much below its usual temperature, the readings being 10°·4 C. and 12° C.; on these occasions the oxygen absorbed per minute was 27 c.c. and 24·8 c.c. respectively, and the mean ratio between the CO_2 produced and O absorbed was $\frac{25 \cdot 9}{228} = 0 \cdot 114$; while when the laboratory was comfortably warm, at a mean temperature of 16°·8 C., the mean volume of oxygen absorbed was 32·4 c.c., and the ratio $\frac{32 \cdot 4}{213} = 0 \cdot 152$. Hence, again, the ratios were lower in low than in high temperatures, which means that in my case in low temperatures less oxygen is absorbed, relatively to the carbonic acid produced, than in high temperatures. The experiments *while fasting* reported in my former paper were all made when the laboratory was comfortably warm, and cannot be utilised towards showing the influence of temperature.

With Mr. Darnell Smith, who kindly submitted to experiment, the results are as follows :—

Table showing the Influence of Changes of Temperature and of the Ingestion of Food on the CO_2 produced and O absorbed per minute. (Darnell Smith, aged 23, under experiment.)

	Duration of experiment.		Time after a full meal.		Temperature of the air.	O consumed.	CO_2 produced.		O absorbed.	Ratio of O consumed to CO_2 produced.
Under the Influence of Food.										
	m.	s.	h.	m.	°	c.c.	c.c.		c.c.	
Means of two experiments ..	8	5	2	55	13·1	275	222		51·9	0·811
,, ,, ..	8	5	2	20	13·2	289	229	238	60·6	0·792
,, ,, ..	8	6	2	35	13·9	289	240		40·3	0·863
,, ,, ..	8	4	2	25	13·9	283	233	231	50·0	0·824
,, ,, ..	7	33	2	55	15·0	288	229		59·0	0·795
,, ,, ..	8	34	2	35	16·7	263	221	222	41·5	0·842
,, ,, ..	7	44	2	33	22·2	270	223		46·9	0·826
Means	8	11	2	30	15·4	280	229		50·0	0·822
Fasting.										
Means of two experiments ..	8	30	5	15	12·9	246	211		35·8	0·855
,, ,, ..	8	32	5	15	15·8	257	225		32·2	0·875
,, ,, ..	8	59	5	40	17·2	246	212		34·2	0·861
Means	8	40	5	23	15·3	250	216		34·1	0·864

In Mr. Smith's case there are actually seven pairs of experiments (fourteen altogether) made under the influence of food. Each experiment was repeated at the time, and the means of the pairs were taken and recorded.

Grouping these pairs of experiments two-by-two, as shown in this last table, the volumes of oxygen consumed will be found to exhibit a slight decrease as the temperatures rise, the carbonic acid produced also shows a slight tendency to fall with the increase of temperature. The oxygen absorbed, which in my case is unmistakably increased with a rise in temperature, is not found to undergo a similar change, but remains nearly the same throughout; there being a slight fall at the two highest temperatures.

With reference to the influence of food on the interchange of the pulmonary gases, the experiments made on Mr. Smith fasting, or when a desire for food was felt about five hours after breakfast, showed a considerable falling off in the oxygen consumed, carbonic acid produced, and oxygen absorbed. Corresponding experiments have been reported in my former paper. Mr. Russell, who assisted me at the time, a young gentleman twenty-one years of age, also exhibited a decrease, though but slight, of oxygen absorbed while fasting, the figures being 33·3 c.c. while fasting, and 37·5 at a mean period of 2 hrs. 16 min. after a full meal. In my case the figures were 35·3 c.c. while fasting and 30·2 under food, which show a slight influence in the opposite direction. It appears, therefore, that the influence of food upon the absorption of oxygen varies with different persons, but that in young and strong people the absorption of oxygen has a decided tendency to increase after a meal.

The results obtained from the present inquiry may be summarised as follows:—

1. Nitrogen acts a part inappreciable, if concerned at all, in the interchange of pulmonary gases.

2. The influence of changes of the atmospheric temperature on the oxygen consumed, carbonic acid produced, and oxygen absorbed, although in the present experiments this influence is more or less interfered with by the phenomena of digestion, is distinct in my case; the oxygen consumed and carbonic acid produced increasing with falling temperatures, while the oxygen absorbed is lessened; and a similar result is obtained from the consideration of the figures given in my former communication. This fact is interesting, perhaps, mainly as showing that the oxygen absorbed is not concerned in the formation of heat in the body towards resisting cold—a function which appears to be limited entirely to the oxidation of carbon into carbonic acid. If, as usually admitted, the blood is limited in its power of taking up oxygen, although this power may apparently vary according to temperatures, then it is obvious that an increased formation of carbonic acid

must necessarily be attended with a falling off in the oxygen absorbed. Cold would thus rob the body of a portion of its oxygen and interfere with those functions of nutrition with which the absorbed oxygen is concerned. This would explain the hibernation of animals—their functions are arrested because the whole of the oxygen they consume in winter is used up towards the formation of carbonic acid, and there is none left to carry on the phenomena of nutrition. It would also account for the sleepiness or stupor well known to be produced by intense cold.

When Mr. Smith was under experiment the influence of temperature on the oxygen consumed and carbonic acid produced was in the same direction, though much less marked than in my case. The oxygen absorbed does not, however, show the same tendency to decrease with a falling temperature. This does not, I consider, invalidate the result as obtained on myself, the temperature of the air being higher when the experiments were made on Mr. Smith than when they were undertaken on myself, and the body being under the influence of an early spring season in March; while the experiments on myself were made in winter, when the laboratory at times was very cold.

3. The influence of food on the interchange of respiratory gases, although being attended with a rise in the oxygen consumed and carbonic acid expired, apparently varies with reference to the oxygen absorbed. Young and strong persons, requiring a full allowance of food, appear to absorb more oxygen while under the influence of a meal than while fasting, but late in life the oxygen absorbed appears to show little or no tendency to increase after a meal. According to Hanriot and Richet, the carbonic acid produced increases considerably under the influence of food, while the increased absorption of oxygen is but slight.*

In conclusion, I wish to allude shortly to a result embodied in my former communication, and relating to the respiration of air containing an increased proportion of carbonic acid. Five experiments had been made—three on myself and two on my assistant, Mr. Russell. The proportions of CO_2 in the air inspired were respectively 2·13, 3·14, 4·06, 3·79, and 3·91 per cent. It was found that the amount of carbonic acid produced in a given time was lower than when pure air was breathed, and that the oxygen absorbed was greatly increased.

The following table gives the figures obtained for the oxygen absorbed:—

* 'Ann. de Chimie et de Physique,' April, 1891.

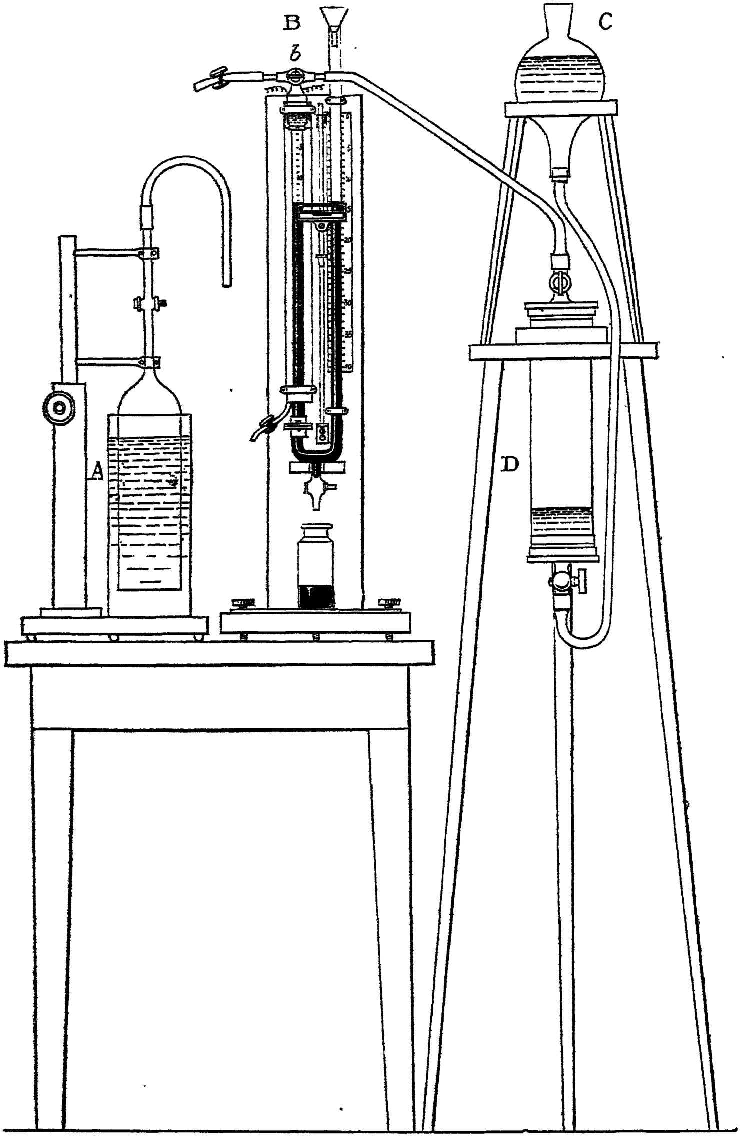
	Per cent. CO_2 in air inspired.	Oxygen absorbed per minute.	Oxygen absorbed under normal conditions per minute.	Oxygen absorbed retained in the blood as CO_2 per minute.
Self 1	2·53	71 c.c.	27	44 c.c.
Self 2	3·54	112 „	27	85 „
Self 3	4·06	127 „	25	102 „
Mr. Russell 1.	3·79	141 „	25	116 „
Mr. Russell 2.	3·91	170 „	37	133 „

It will be seen that the oxygen absorbed per minute when carbonic acid was breathed greatly exceeded the oxygen absorbed when pure air was inspired. The figures given in the table for O absorbed under normal conditions, are taken, with one exception, from experiments made at the same sitting as those in which CO_2 was breathed, and therefore compare with them. The excessive volume of oxygen absorbed when air mixed with CO_2 was inhaled must have been owing to the fact that a portion of the oxygen consumed, which under usual circumstances would have been given out as carbonic acid, remained in the blood, obviously as carbonic acid, because of the impeded diffusion in the lungs on account of the CO_2 contained in the air breathed. The actual amount of carbonic acid being thus retained in the blood can be calculated, with what I think a near degree of approximation, by subtracting the oxygen which would have been absorbed in the respiration of pure air from the oxygen actually found to have been absorbed under the influence of the CO_2 inspired.

These figures, varying from 44 c.c. to 133 c.c., are entered in the last column of the table, and show how large a volume of CO_2 may be retained in the blood when air contaminated with CO_2 is used for respiration. This carbonic acid goes on accumulating in the body interfering with the normal phenomena of oxidation. It is very difficult to consider this effect otherwise than poisonous; although under circumstances which admit of the introduction of CO_2 in the blood through another channel than respiration, the gas appears innocuous. Cl. Bernard has shown this to be the case by the injection of CO_2 into the circulation of animals, when the gas found its way out of the body through the lungs without producing any morbid action.

Description of a New Form of Eudiometer.

A. Receiver containing hydrogen gas.
B. Eudiometer.
b. Three-way stopcock.
C. Flask holding water.
D. Cylinder containing the gas for analysis.



The eudiometer has the form of a U-tube, drawn into an open neck at its bend, where the opening is controlled with a stopcock. One of the limbs of the instrument, which is graduated, is surrounded by a water jacket, and has an iron cap cemented to it, in which there is a

three-way iron stopcock. A scale is fixed at the back of the open limb, the divisions corresponding exactly to those engraved on the other limb.

The flask C, open at both ends, is full of water, and communicates by india-rubber tubing with the cylinder D, which contains the air for analysis.

The process of analysis is as follows:—The eudiometer is entirely filled with mercury. Next, the hydrogen being placed under a pressure of 1 or 2 inches of water by depressing the receiver, the gas is driven through the three-way cock, while at the same time it is aspired by a compressed india-rubber syringe, which is suddenly released; this washes out all the atmospheric air from the tube connecting the supply of hydrogen with the eudiometer. The three-way cock being turned so as to connect the hydrogen-receiver with the eudiometer, mercury is let out at the bend of the instrument, when hydrogen is drawn into it, and the volume of the gas recorded. Air from the flask D is admitted into the eudiometer through the india-rubber tube, pressure being exerted by the water held in the flask D, while aspiration is produced by letting out mercury from the eudiometer. The gases are brought under atmospheric pressure by adding or taking out mercury.

The gases are mixed in the eudiometer by means of an india-rubber syringe fitting to the open end of the instrument, where the funnel is shown in the drawing; the syringe is compressed and released repeatedly, thus driving the mercury up and down. The little movable spirit level is convenient, though not indispensable, towards the adjusting of the mercury in the two limbs. I find that such an instrument has been proposed, or used, for similar purposes by G. Lunge ('Chem. Soc. Jl.,' 1892), but the readings are not so reliable as those obtained from two identical scales, one for each limb of the U-tube.

The gases are finally exploded in the usual way.

The process is described in full in my paper read June, 1891, to the Royal Society.

"Magnetic Properties of Pure Iron." By FRANCIS LYDALL and ALFRED W. S. POCKLINGTON. Communicated by J. HOPKINSON, F.R.S. Received May 4,—Read June 16, 1892.

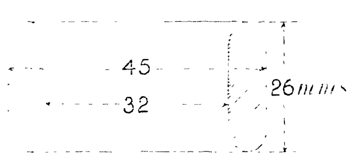
The following results were obtained at King's College, Strand, for a specimen of very pure iron. The experiments were made under the direction of Dr. Hopkinson. The sample was supplied to him by Sir Frederick Abel, K.C.B., F.R.S., to whom it was sent by Colonel Dyer, of the Elswick Works. It is of almost pure iron, and the substances other than iron are stated to be:—

Carbon.	Silicon.	Phosphorus.	Sulphur.	Manganese.
Trace.	Trace.	None.	0·013	0·1

The method of experiment is the same as that described in Dr. Hopkinson's paper before this Society on the "Magnetisation of Iron at High Temperatures," viz., taking a curve of induction at the temperature of the atmosphere, and then at increasing temperatures until the critical point is reached. The temperatures, as in his paper, are calculated from the resistances of the secondary winding, the increase of resistance per 1° C. being assumed to be 0·00388 of the resistance at 20° C. In brackets are also given the temperatures calculated by Benoit's formula—

$$\text{Resistance at } t^\circ \text{ C.} = \text{resistance at } 0^\circ \text{ C.} \{1 + 0{\cdot}00367t + 0{\cdot}000000587t^2\}.*$$

The dimensions of the iron ring are—

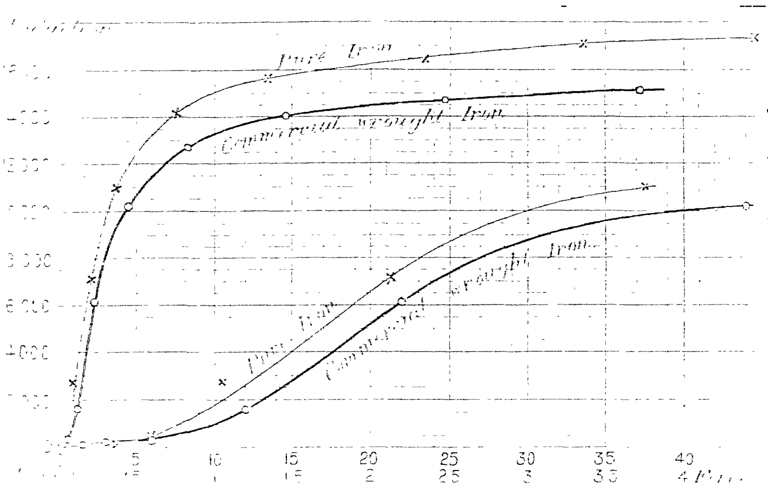


as in the earlier experiments.

Fig. 1 gives the curve of induction taken at 10°·5 C. compared with the sample of wrought iron of Dr. Hopkinson's paper, just referred to, taken at 8°·5 C. It shows the very high induction developed in the pure specimen for a moderate magnetising force, and also the small amount of hysteresis. The following are the actual values of induction, B, and magnetising force, H:—

* Everett's 'C.G.S. Units and Physical Constants,' p. 160.

FIG. 1.



Resistance of secondary = 0·75 ohm. Temperature, 10°·5 C. (pure specimen, marked ×).

B...	34	118	467	2700	7060	10,980	14,160	15,590	16,570	17,120	17,440
H...	0·15	0·38	0·6	1·06	2·11	3·77	7·48	13·36	23·25	33·65	44·66

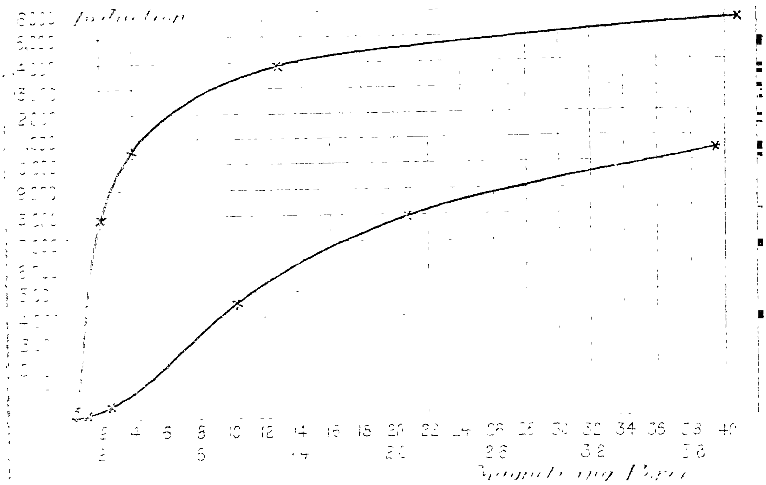
Temperature, 8°·5 C. (ordinary specimen, marked ○).

B.....	39·5	116	329	1560	6041	10,144	12,633	14,059	14,702	15,149
H.....	0·15	0·3	0·6	1·2	2·2	4·4	8·2	14·7	24·7	37·2

Figs, 2, 3, 4, and 5 give the curves taken at the following temperatures, as calculated from the secondary resistances—658° (676°), 727° (738°), 770° (780°), 855° (857°).

The values for these curves are as follows :—

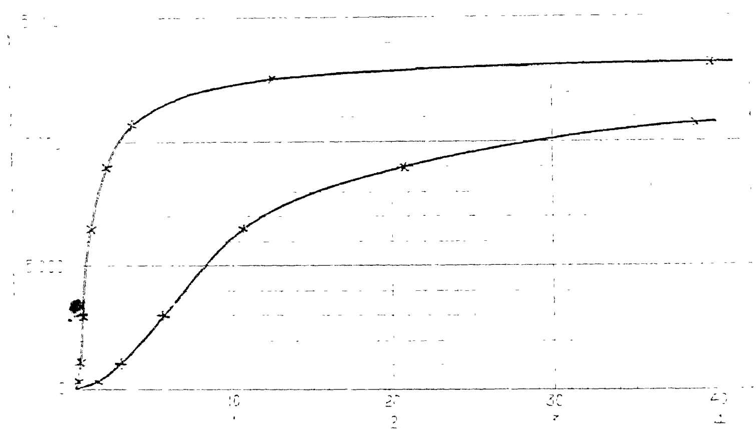
FIG. 2.



Secondary resistance = 2·706 ohms. Temperature, 658° C. (676°).

B.........	103·37	360	4453	7899	10,556	13,836	15,640
H.........	0·09	0·25	1·02	2·08	3·97	12·96	40·92

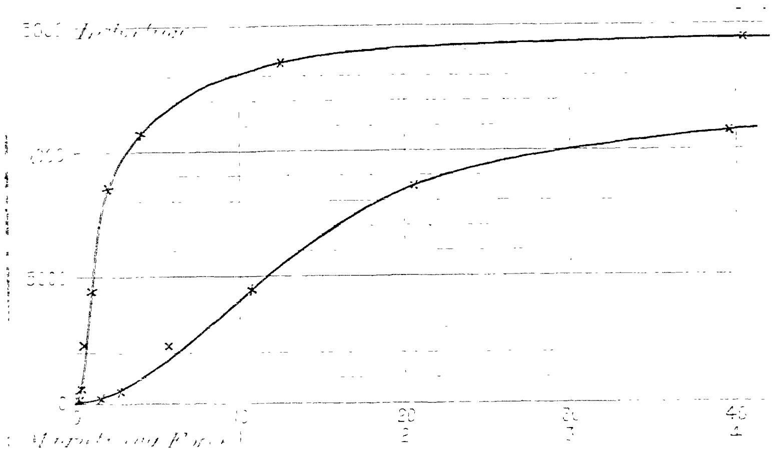
FIG. 3.



Secondary resistance = 2·91 ohms. Temperature, 727° C. (738°).

B...........	167	532	2260	4405	8553	10,763	13,580	14,442
H...........	0·15	0·28	0·56	1·08	2·05	3·97	12·62	40·4

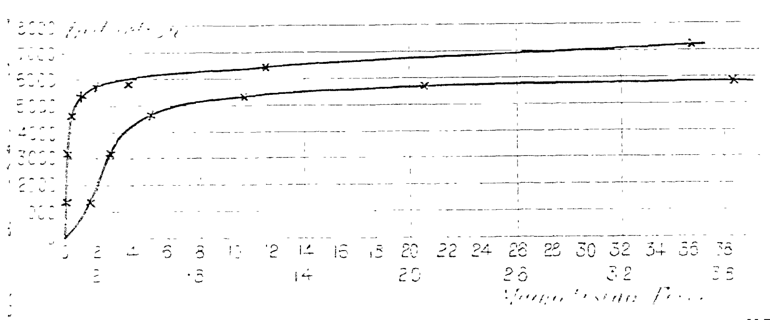
FIG. 4.



Secondary resistance = 3·046 ohms. Temperature, 770° (780°).

B...........	249	1030	2971	6441	8944	10,727	12,528	13,139
H...........	0·14	0·28	0·56	1·07	2·08	3·87	12·6	39·7

FIG. 5.



Secondary resistance = 3·303 ohms. Temperature, 855° C. (857°).

B.........	1316	3123	4682	5347	5779	5902	6513	7139
H.........	0·15	0·28	0·53	1·05	2·08	3·87	11·9	36·1

In these we see for a rise of temperature a marked decrease of hysteresis and a very much lower maximum of induction.

Also that for a small magnetising force the permeability rises very remarkably with the temperature, but just the reverse for a force of, say, "40."

FIG. 6.

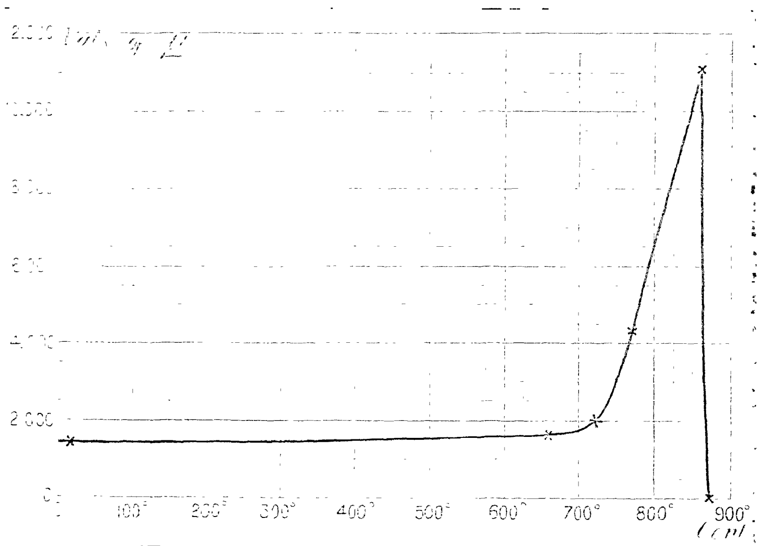


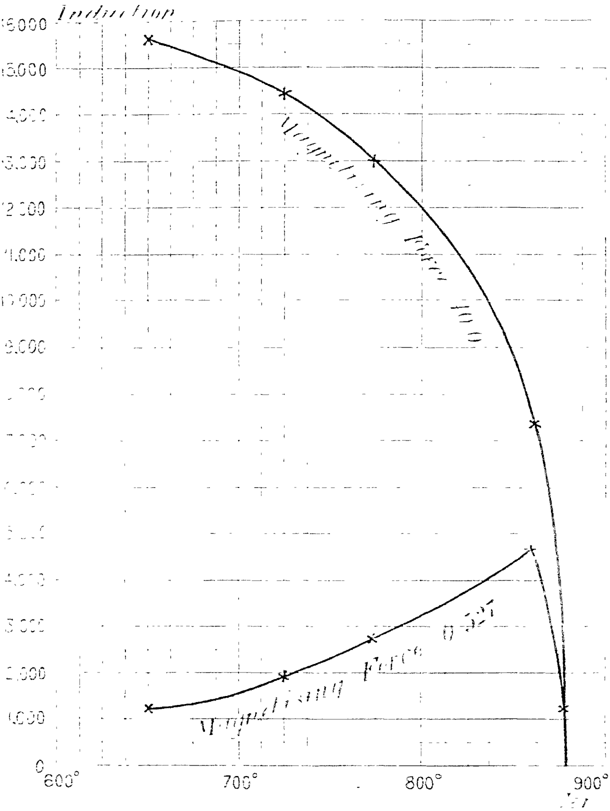
Fig. 6 shows the rise of permeability in relation to temperature when $H = 0{\cdot}3$, the maximum permeability observed being 11,100 for a magnetising force of 0·3, and at a temperature of 855° C. (857°).

Fig. 7 contrasts the relation of induction to temperature at a small and a larger magnetising force.

During the heating of the specimen, the critical point, when the iron suddenly became non-magnetic, was reached at 874° C. (875°), and on cooling it became magnetic at 835° C. (838°).

Comparing these results with those obtained with the more ordinary specimens of iron mentioned in Dr. Hopkinson's paper, we have here 874° C. as against 786° C., while in an experiment on some soft iron wire the critical temperature was 880° C., and for hard piano-forte wire it was 838° C.

FIG. 7.



"On the Shoulder Girdle in Ichthyosauria and Sauropterygia." By J. W. HULKE, F.R.S. Received April 11,—Read May 12, 1892.

In a paper recently communicated to the Royal Society by Professor H. G. Seeley,* in which is discussed "The Nature of the Shoulder Girdle and Clavicular Arch in Sauropterygia," the author challenges the validity of statements relating to this girdle made by me at the Anniversary Meeting of the Geological Society of London in 1883.† This from so eminent a palæoherpetologist imposed on

* Seeley, Professor H. G., "The Nature of the Shoulder Girdle and the Clavicular Arch in Sauropterygia." Received January 18, 1892. Read February 18, 1892. 'Proc. Roy. Soc.'

† Hulke, J. W., "Presidential Annual Address," 'Geol. Soc., Lond.,' on Feb. 16, 1883; 'Quart. Jl. Geol. Soc.,' vol. 39, 1883, Proc., p. 38.

me the task of re-examining the facts and the considerations on which were based those statements made nine years ago, and now questioned. The outcome of this enquiry I now offer to the judgment of the Royal Society.

In his recent paper, in addition to the known elements recognised by all *comparative anatomists* (*viz., two scapulæ, two coracoids, two* clavicles, and an interclavicle), Professor H. G. Seeley (*a*) assigns to the Ichthyosaurian shoulder girdle a *precoracoid*, cartilaginous; (*b*) he regards as non-proven my interpretation of the anterior of the two ventral rays present in the Plesiosaurian girdle, as a *precoracoid;* (*c*) he considers that the process ascending from the body of the scapula in *Plesiosauria* is not homologous with the analogous vertical ray in the Testudinate shoulder girdle; and (*d*) he contends that certain osseous components of the Plesiosaurian girdle are *clavicles* and *interclavicle*, and not *omosternalia.*

It will be convenient to notice these matters serially.

(*a*) The value of the evidence on which a *precoracoid*—cartilaginous—has been assigned to the *Ichthyosaurian* shoulder girdle.

Professor H. G. Seeley conceives the presence of a precoracoid—cartilaginous—in this girdle which "articulated with the part of the scapula anterior to the external articulation of the coracoid, and also with the anterior interior processes of the coracoids, so as to complete the precoracoid foramen anteriorly."

He founds this conception mainly on the supposition that the ventral end of the scapula comprises three several portions:—(1) a posterior portion which contributes to form the *fossa glenoidalis;* (2) a middle portion articulating "with the anterior, articular edge of the coracoid;" and (3) an anterior which does not differ in its cartilaginous, articular aspect or thickness from the middle portion, but which looks inwards without any other bony element of the shoulder girdle to articulate with it.

The conception of a distinct anterior portion of the ventral end of the scapula in front of that part of this end of the scapula which articulates with the coracoid, therefore which is in front of the scapulo-coracoid articulation, is, then, a principal reason for Professor H. G. Seeley's conception of a precoracoid—cartilaginous—in the Ichthyosaurian girdle.

A careful study of many Ichthyosaurian scapulæ disposes me to regard as fallacious the appearance of a tripartite division of the ventral end of the bone; such a division does not appear to me to be supported by the best preserved and most perfect examples.

In a scapula, now before me, obtained by A. Leeds, Esq., from the clay pits (Oxford clay) near Peterborough, which retains its normal figure, texture, and surface markings, as perfectly as a newly mace-

rated bone, I find the ventral end to comprise a stout, posterior part of an approximately semi-ellipsoidal figure, having its major axis, 10·5 cm. long, much nearer the inner border, which does not greatly vary from a straight line. The minor axis, 4·3 cm. long, is distant 4·6 cm. from the posterior vertex of the ellipsoid, towards which the thickness of the bone rapidly diminishes. The reduction of thickness forwards is less rapid, the width across the end, between the inner and the outer surface, being 1·8 cm., at the anterior vertex of the ellipsoid. From this point forwards the thickness of the bone continues to decrease to the anterior border of the bone, with which this part of the ventral end makes an angle. At its anterior termination here the thickness of the ventral end is only 0·8 cm. The surface texture of this end has a granular character suggestive of a former cartilaginous crust. This granulation is coarser in the narrower anterior part.

When this scapula is stood on its ventral end on a flat horizontal surface, as on a table, and viewed perpendicularly to one of its surfaces (preferably the inner surface), the profile of this end of the bone comprises a middle, horizontal segment, from both ends of which the contour rises angularly; its posterior branch includes with the horizontal direction of the middle part (produced) an angle of about 35°; and its anterior branch includes with the same horizontal line an angle of 65°.

This different direction of the parts of the profile apparently has suggested to Professor H. G. Seeley a threefold division, in which each part had a distinct separate office. A critical examination of the surface of the end shows, I think, that the idea of a tripartite division of this end is illusory, and that this end comprises only two parts, one posterior, glenoid, diarthrodial segment; the other, an anterior synchondrosial segment, which articulated with the coracoid.

The outer surface of the scapula near its ventral end is sinuous; convex in its posterior third, where the bone is stoutest; and concave in its anterior two-thirds, where the bone becomes thin, the concavity here being chiefly due to the *outward* trend of the surface towards the anterior border which gives to this part a thin, lip-like, angularly projecting figure where the ventral and anterior borders meet.

In conformity with the ventral end of the scapula, the corresponding outer border of the roughly quadrilateral coracoid exhibits a stout posterior part, its complement of the *fossa glenoidalis;* and a thinner anterior part which encroaches on the anterior border by truncation of the antero-external angle, for synchondrosial union with the scapula.

Bearing in mind that the bones composing the Ichthyosaurian

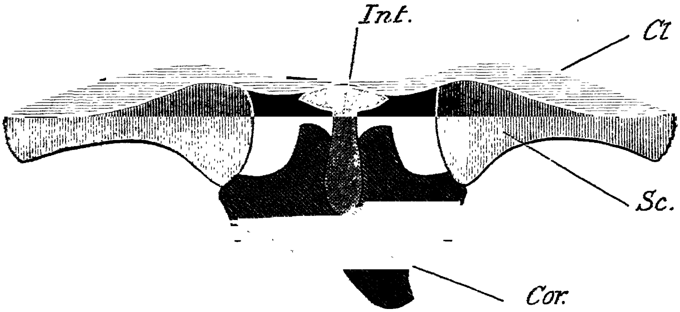


Fig. 2.—Conybeare's reconstruction of pectoral girdle of *Ichthyosaurus* ('Trans. Geol. Soc. Lond.,' vol. 1, Part 2, Series 2, Pl. 49, fig. 7. Copied in 'Oss. Foss.' Edit. 3, Tome 5, Pl. 32, fig. 7).

tions of the Ichthyosaurian shoulder girdle differ greatly, notably so in the positions and the relations of the scapula. As, however, Cuvier copies both, and abstains from expressing his own views on the subject, obviously little weight attaches to his authority in regard to it.

The passage quoted above from the "Ossemens Fossiles," certainly mentions a "*proéminence*" on the anterior border of the scapula for affording support to the end of clavicle; but the passage is not quite free from ambiguity. It does not, however, appear to me to fix the position of the scapula shown in Sir E. Home's reconstruction, and adopted by Professor H. G. Seeley, which represents a non-articular part of the ventral end of this bone in advance of the coraco-scapular articulation; for the reconstruction shows the mesial ends of the clavicles resting on the anterior aspect of the cross-bar of the interclavicle, as we know by numerous examples they certainly do in many *Ichthyosauria*, and not on the antero-ventral angle of the scapula; so that the end of the clavicle here referred to by Cuvier would seem not to be the inner but the outer end. Where this rests on the anterior border of the scapula, I have noticed in some examples a rough, low elevation, as if for a ligamentous attachment, a circumstance confirmatory of the idea that the outer and not the inner end of the clavicle is here intended.

Buckland, another authority cited by Professor H. G. Seeley in support of his view respecting the ventral end of the scapula, reproduces Cuvier's reduction of Sir E. Home's reconstruction given in fig. 1, Pl. 2, 'Phil. Trans.,' 1818; and since in his text he omits all detailed description of the several parts composing the shoulder girdle, he leaves us in ignorance whether he had himself formed a distinct opinion respecting the normal position and relations of the Ichthyosaurian scapula.*

* Buckland, W., 'Geology and Mineralogy considered with Reference to Natural Theology,' p. 181, vol. 1, 1836, and Pl. 12.

Obviously, then, there is small claim to quote Buckland as an authority upon the anatomical details of the pectoral arch.

Amongst other reasons for suggesting the presence of a precoracoid—cartilaginous—in *Ichthyosaurus* assigned by Professor H. G. Seeley, are the following, viz.:—

1. "That it accounts for the structure of the shoulder girdle, and explains its homology."

If by "the structure of the shoulder girdle" the author means, as I imagine, the alleged tripartite division of the ventral end of the scapula, and the marks of attachment of cartilage apparent in the foremost of the supposed three divisions, and also on the antero-internal angle of the coracoid, both are naturally explained in the suggestion I have offered above, and this without the introduction of another skeletal element of which no objective trace remains. As regards the homologies of all the known parts of the Ichthyosaurian shoulder girdle, I had supposed that all comparative anatomists had long been in perfect accord.

2. "It brings the shoulder girdle of *Ichthyosaurus* into harmony with *Nothosaurus*, in which there is a similarly incomplete coracoid foramen, and similar cartilaginous surfaces of coracoid and scapula in close juxtaposition."

The Nothosaurian (osseous) shoulder girdle, for the knowledge of which we are indebted to H. v. Meyer,* comprises three pairs and an azygos piece, viz., two coracoids, two scapulæ, two clavicles, and an interclavicle.

These are precisely the same osseous parts as are present in the Ichthyosaurian shoulder girdle.

Now, Professor H. G. Seeley inferring the existence of a precoracoid in *Ichthyosaurus*, supports this hypothesis by the argument that its presence would bring the Ichthyosaurian girdle into harmony with that of *Nothosaurus*, in which its presence is equally hypothetical.

The form and the relations of its osseous parts are well seen in the annexed figure from Zittel's 'Palæontologie,' which is a reduction of one-fourth of H. v. Meyer's large figure of the shoulder girdle of *Nothos. mirabilis.*†

H. v. Meyer regarded the two angulated long bones composing the principal part of the anterior ventral ray as clavicles, and the small middle piece embraced by their inner ends as "sternum" (interclavicle in the nomenclature of to-day). I formerly thought these precoracoids, but better knowledge of the relation of their inner ends to the interclavicle, and of their outer ends to the scapula, has

* Meyer, H. v., Saurier des Muschelkalks ('Zur Fauna der Vorwelt,' 1845—1857), Fig. 1, Lief. 34, Fig. of Nothos. Shoulder Girdle viewed from above. Natural size.

† Zittel, K. A., 'Handbuch der Palæont.,' vol. 3, Abth. 1; S. 476, fig. 447.

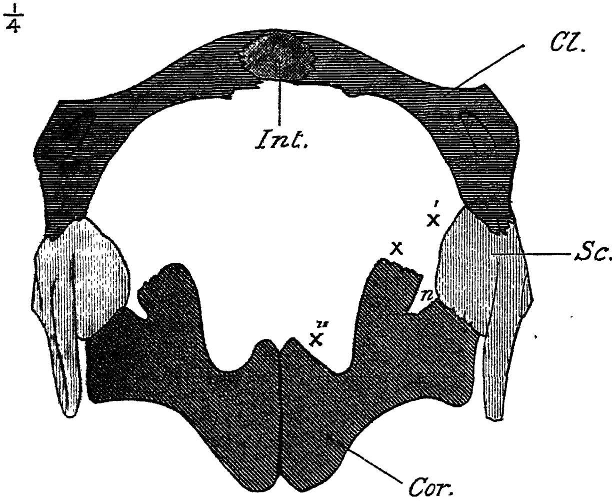


FIG. 3.—Shoulder girdle of *Nothosaurus mirabilis*. $\frac{1}{4}$ natural size. From Zittel's 'Handbuch der Palæont.,' Bd. 3, Lief. 3, S. 476. (This is a reduced copy of H. v. Meyer's, Fig. 1, Taf. 34, 'Saurier des Muschelkalks.')

long appeared to me conclusive of the correctness of H. v. Meyer's interpretation of their homology; and I am confirmed in this rectification of my earlier view by instances of a similar behaviour of the inner ends of the clavicles and interclavicle in certain *Ichthyosauria* from the Oxford clay obligingly brought under my notice by A. Leeds, Esq., of Peterborough. Such relations obtain in *Ophthalmosaurus icenicus* (Seeley, H. G.) ('Geol. Soc. Quart. Jour.,' p. 396, *et seq.*, vol. 30, 1874).

Whilst in certain *Ichthyosauria* the relations of the inner ends of the clavicles and interclavicle are in close accord with those obtaining in *Nothosaurus*, the relation of the outer ends of the clavicles to the scapulæ is different.

In *Ichthyosauria* the clavicle tapers laterally, not widening, and its outer end creeps up along the anterior border of the blade of the scapula, splint-wise, nearly or quite to the full extent of the latter, with which, as already mentioned, it seems to have been only ligamentously connected. In *Nothosaurus*, however, the outer end of the clavicle is united suturally* to the body of the scapula, and at the root of its ascending process, which most comparative anatomists, I suppose, identify with the blade of the scapula.

* The firmness of the suture (serrated) is shown by the fact that von Meyer found several specimens in which the clavicle had broken off near the suture which still held a fragment of the clavicle firmly knit to the scapula.

Unless I have greatly failed to grasp his meaning, Professor H. G. Seeley grounds his idea of the presence of a cartilaginous precoracoid in *Nothosaurus* on the characters of the terminal border of the process marked x, and on that of the part of the scapula marked x' (fig. 3), and also in the presence of the notch n, at the outer side of the process x, which he identifies with the notch in the anterior border of the Ichthyosaurian coracoid, and so with the *precoracoid* foramen (*cor. for.* of most authors) in *Reptilia squamata*. Were the notch n converted into a foramen by a cartilaginous band joining x and x', such cartilaginous bar with the bony process x would constitute the precoracoid element conceived by Professor H. G. Seeley. This hypothesis is not free from difficulty. In both the primary divisions of Amphibia the cartilaginous precoracoid is more in line with the scapula, forming, as it were, a ventral extension of this, than it is with the precoracoid, and when the precoracoid ossifies (Urodela) in common with either of the other components of the girdle, ossification appears to overrun it from the scapula rather than to extend into it from the coracoid. But in *Nothosaurus*, on the supposition that the process x is part of the precoracoid, obviously, ossification has spread into this from the coracoid and not from the scapula, with which also it has no community of direction. Next, as regards the supposed identity of the notch n, at the outer side of the process x, with the foramen of nerve-passage in the Lacertilian coracoid. This in all lizards I have examined with particular reference to its position is situated behind and usually slightly towards the mesial side of the precoracoid tract or process, so that the homologous notch in the Nothosaurian girdle is to be sought at x'', and not at the outer side of x.

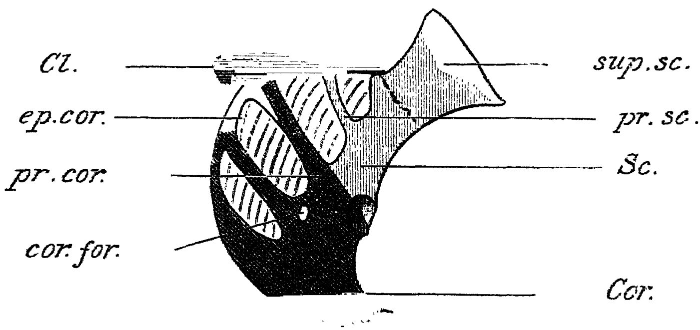


FIG. 4.—Left half of shoulder girdle of *Iguana tuberculata*.

But if we were to concede to Professor H. G. Seeley the presence of a *pre*-coracoid in the Nothosaurian shoulder girdle joining x and x', its relation to the coracoid would be different to that which he assigns

to it in the Ichthyosaurian girdle in which he connects the mesial end of the precoracoid with the antero-internal angle of the coracoid; whereas in *Nothosaurus* its mesial end would be separated from that angle by a wide interval.

That the process *x* was tipped with cartilage, and that the part of the scapula marked *x'* was clothed with cartilage is an inference not drawn by H. v. Meyer himself who, from a study of several specimens, says, the anterior border of the process *x*—"bietet auf seinem Vorderrande Unebenheiten dar, welche auf ein an dieser Stelle angebracht gewesenes Band schliessen lassen;" and of *x'* that the border is—"von einer Beschaffenheit welche vermuthen lässt, dass sie mit einem Band oder Muskel in Berührung gestanden hat."*

From the above I think it will have become evident that the grounds for the supposition of a precoracoid (cartilaginous) in *Nothosaurus* are insufficient to establish this; and if so, that the structure of its shoulder girdle as regards this element fails to support the hypothesis of a precoracoid in Ichthyosauria.

G. Baur has "no doubt that the Ichthyosauria possessed a small *cartilaginous sternum.*" He adds "the whole morphology of the shoulder girdle strongly supports this opinion."† I am pleased to find myself in accord with him on this matter, for I have long recognised the great probability of the presence of a cartilaginous sternal plate behind the coracoids. I also have no doubt that the Ichthyosauria had a cartilaginous supra-scapula.

In connexion with the hypothesis of a precoracoid in the shoulder girdle of *Nothosaurus mirabilis*, the small Nothosaurid *Lariosaurus Balsami Curioni* deserves notice. Its shoulder girdle contains the same elements as those in *N. mirabilis*, and their forms and arrangements are similar, the chief difference being the absence of the large process so conspicuous at the anterior margin of the coracoid in the latter. In the more simple form of this bone there is a closer resemblance to Plesiosauria than is shown by *N. mirabilis*. The annexed figure from C. Zittel's 'Pal.' affords the means of comparison of the girdles of *Lariosaurus Balsami* with those of *Plesiosaurus* and *N. mirabilis*. In an upper view of a complete skeleton of *Lariosaurus Balsami* (original in Munich Museum), figured by C. Zittel, 'Pal.,' vol. 3, p. 485, fig. 461, the dorsal process of the scapula is seen in natural position above the glenoid mass, rising with a strong backward slant (*d.pr.*).

In *Anarosaurus pumilio* (W. Dames) another small member of the same family, the coracoid also has a simple form, and its anterior border wants the process characteristic of *N. mirabilis*. Again, in *Neusticosaurus pusillus* (H. G. Seeley), another member of this family,

* Meyer, H. v., *op. cit.*, p. 45.

† Baur, G., 'On the Pelvis of the Testudinata.' Boston, 1891. Reprint from the 'Journal of Morphology,' vol. 4, No. 3, p. 34.

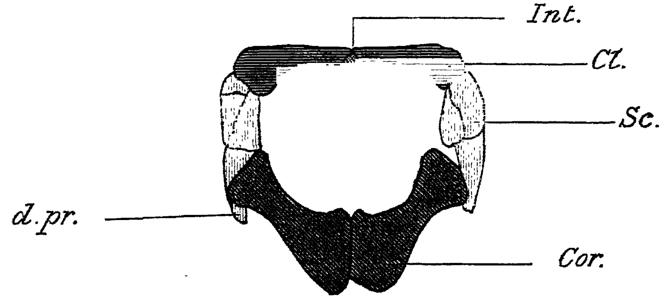


FIG. 5.—Shoulder girdle of *Lariosaurus Balsami Curioni* (ventral view). From Zittel, *op. cit.*, S. 488, fig. 462. *d.pr.*, dorsal process of scapula.

the relations of the anterior ventral ray and scapula are in principle identically the same as in *N. mirabilis;* the anterior ray articulating with the scapula, and not contributing to form the fossa glenoidalis. The coracoid, however, repeats the simpler form seen in *Lariosaurus Balsami* and in *Anarosaurus pumilio.*

Thus if *Macromerosaurus* be only a young example of *Lariosaurus*, and if *Pachypleura* is *Neusticosaurus*, as C. Zittel, W. Dames, and some others suppose, we have the fact that none of the family of Nothosauridæ, unless *N. mirabilis*, lend any support to the idea of the existence of a precoracoid, but rather the contrary; and those points in *N. mirabilis* which have been cited in support of the presence of a precoracoid in this, and so in *Ichthyosaurus*, are, as I have suggested, capable of other and simpler explanation.

Another reason assigned by Professor H. G. Seeley for introducing a cartilaginous precoracoid into the Ichthyosaurian shoulder girdle is "it brings the shoulder girdle of *Ichthyosaurus* into harmony with that of the Anomodontia, because they correspond in the form of the scapulæ, the position and forms of the clavicles, interclavicles, and coracoids; so that if the Anomodont precoracoid were unossified the differences from *Ichthyosaurus* would be small, except that the Anomodonts develop an epiclavicle of Labyrinthodont type."

The principal Anomodont remains available for the comparison are, I take it for granted, those preserved in the British Museum, chiefly from African sources. They include several dissociated more or less imperfect scapulæ and coracoids, of which the more important pieces were figured by R. Owen in his 'Catalogue of S. African Reptilia,' 1876, Pl. 69, figs. 5, 6, 8, 9, and Pl. 70, fig. 1. They comprise also the remains of *Pareiasaurus*, described by Professor H. G. Seeley, 'Phil. Trans.,' B. 1888, p. 59, *et seq.*, Pl. 20, figs. 1, 2; those of *Keirognathus cordylus*, described by this author, 'Phil. Trans.,' B,

1888, Pl. 75, p. 489; and those of *Procolophon*, described by the same author, 'Phil. Trans.,' B. 1889, p. 255, Pl. 9, fig. 9.

Now, the most notable character in the best preserved scapula figured by R. Owen (fig. 1, Pl. 70, *op. s. c.*), is the large process at the ventral termination of its anterior border separated by a deep notch from the coracoid articular margin of the bone (fig. 6). This process, though

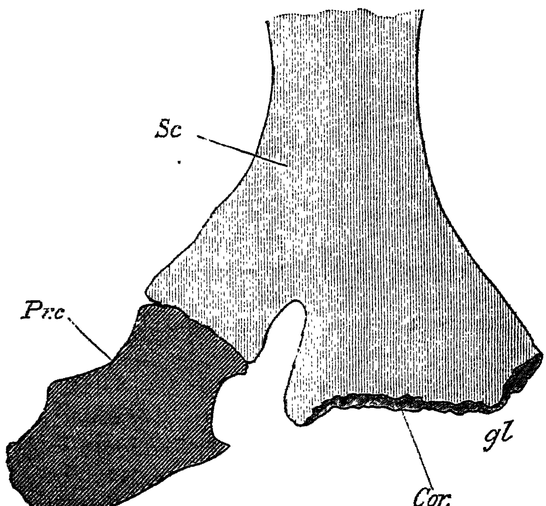


FIG. 6.—Scapula and precoracoid referred to *Dicynodon leoniceps*. Owen, 'S. Afric. Rept.,' Pl. 70, fig. 1. *prc.*, precoracoid; *gl.*, glenoid.

termed acromion by R. Owen and by Professor H. G. Seeley, can hardly be regarded as homologous with the acromion in higher Vertebrata, since in *Dicynodon* it articulates with the precoracoid and not with the clavicle (fig. 7). A similar form of the ventral end of the scapula is not infrequent in Anourous Amphibia, but I am not aware that such has yet been demonstrated in the scapula of any Ichthyosaurian. In *Dicynodon* the clavicles are not known. R. Owen figures (Pl. 49, fig. 8, *op. cit.*) a bone associated with a scapula which he considers interclavicle of *Kistecephalus*. This Professor H. G. Seeley interprets as clavicle. It is dissociated, and the other ventral constituents of the shoulder girdle are missing. I submit, therefore, that no certain information of the clavicle is derivable from this specimen. In *Pareiasaurus*, the interclavicle and the clavicles are known, but not the other ventral elements of the girdle, or, at best, most imperfectly; nor is the form of the scapula known. In *Procolophon*, the interclavicle is well shown in the specimen figured by H. G. Seeley

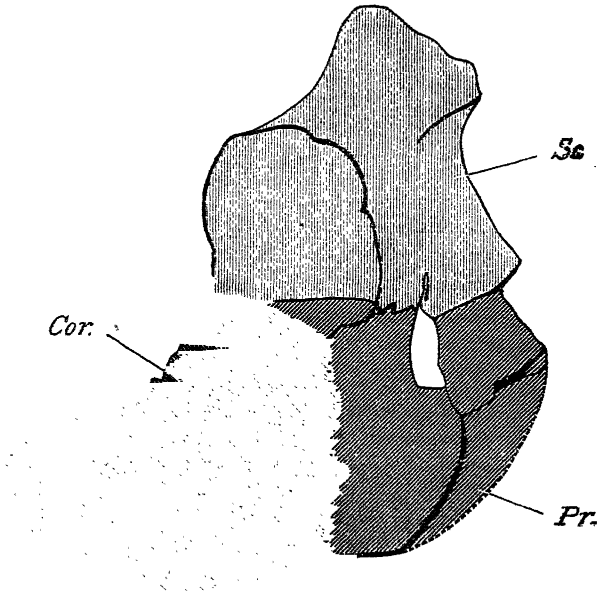


FIG. 7.—Scapula, coracoid, and precoracoid of *Dicynodon* (sp.). Owen, 'S. Afric. Rept.,' Pl. 69, fig. 5.

(fig. 9, Pl. 9, 'Phil. Trans.,' B, 1889). Its form differs notably from that of *Pareiasaurus* in the great length of its sagittal bar, but the clavicle and the scapula are not known. In *Keirognathus cordylus* (figured by Professor H. G. Seeley, 'Phil. Trans.,' B, 1890, Pl. 75), the clavicles are represented by small discontinuous fragments of doubtful interpretation. The bone, with much probability interpreted as interclavicle, is imperfect; its relation to the coracoids, precoracoids, and sternum are unusual. The coracoids and precoracoids are represented only by impressions in the matrix to which but vestiges of bony tissue remain attached: and the scapula is also extremely imperfect.

I submit, then, that our present knowledge of the Anomodont shoulder girdle is too incomplete to serve for any other than a very general comparison with that of *Ichthyosaurus;* and I venture to think that at present it is insufficient to warrant the conclusions of a close agreement in their structural details.

I pass on to the Plesiosaurian shoulder girdle. Since the qualifying prefix *inter-* in interclavicle, and *epi-* in episternum, primarily denote a relation of position and not of genesis, it does not appear to me a matter of great moment which of these names is employed,

and I do not find any fault with those authors who use these names indifferently as synonyms of a certain piece in the secondary shoulder girdle. I prefer, myself, the term interclavicle, since Götte's embryological investigations have appeared to demonstrate that the piece thus denoted is a derivative of the mesial ends of the clavicles. For the analogous part in the Amphibian girdle, since W. K. Parker apparently demonstrated its origin from the epicoracoids (and, if so, it is not the morphological equivalent of the Lacertilian interclavicle), I prefer his name *omosternum*, which has the convenience of implying its essential difference from the interclavicle.

In Plesiosauria the (primary) shoulder girdle, as is well known, consists of a dorsal and of two ventral rays, the *fossa glenoidalis* being seated (approximately) at the spot whence the three rays diverge. In addition to these component parts there are others which, for reasons to be presently stated, I have suggested are omosternalia; if such they are also parts of the primary girdle.

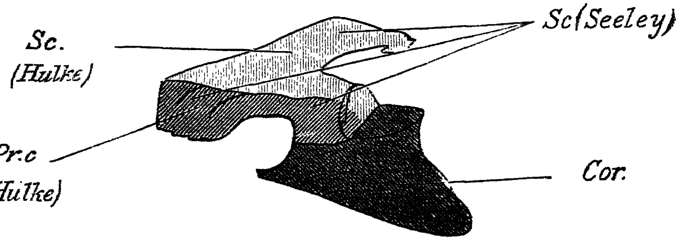


FIG. 8.—Left half of primary shoulder girdle of *Plesiosaurus cliduchus* (Seeley). From photograph of specimen in Woodwardian Museum; taken June, 1869.

Professor H. G. Seeley, however, finds, in these parts, clavicles and interclavicle, a view which, if established, places them in the secondary girdle.*

Concerning the posterior ventral ray, the suggestion that it may include the coracoid and also the precoracoid element has, I think, never been seriously argued, and I believe that all comparative anatomists now agree in regarding it in its entirety solely as coracoid.

After the recognition of "the large flat bone," by Buckland, as the "sternum" in *Ichthyosaurus*, and after its correspondence to the coracoid in Crocodilia had been shown by Conybeare, the latter's identification of the Plesiosaurian coracoid, when soon afterwards this Sauropterygian was discovered by him and De la Beche, almost necessarily followed.†

* C. Gegenbaur denotes by *primary*, that part of the girdle which is preformed in cartilage; and by *secondary*, that which ossifies directly from membrane.

† Conybeare, *op. supra cit.*

The significance of the anterior ventral ray has ever been, and it still is, a "vexed question." I regard it as a precoracoid preserving permanently its original union with the scapula, together with which it shares in forming the *fossa glenoidalis.*

Professor H. G. Seeley considers it part of the scapula. This is plainly shown by the following extracts from his paper:—"The scapula is a stout triradiate bone." "In *Elasmosaurus* the scapular arch has the well-known form with the scapulæ meeting in the median line and continuous posteriorly with the coracoids." "The measurement of the scapula in the median line is $3\frac{3}{4}$ inches." "The scapulæ meet in the usual usual way by a median suture." "The interclavicle was found *in situ* resting in a depression between the anterior margins of the scapulæ." "The interclavicle is wedged in between the scapulæ."

The above passages prove, I submit, that their author rejects the actually dual composition of the bone termed by him, in its entirety, scapula: nor is the significance of these passages weakened by others, as the following, in which the bone is given a compound name, so:— "The scapulo-precoracoid appears to form about two-thirds of the wall of the glenoid cavity." "It is these *precoracoid portions of the scapulæ* which alone meet each other in the median line." That these last quoted passages may not be understood as implying a shade of doubt in the author's mind respecting the genetic oneness of the ray he calls, in its entirety, scapula, appears to be clear from the following passage, on the shoulder girdle in certain Plesiosauroids:—"In *Elasmosaurus, Colymbosaurus, Murænosaurus,* and their allies, the parts of the bone which meet in the median line, and are in contact with the clavicular arch, are *theoretically precoracoid** elements which connect the scapulæ with the coracoids" (*op. supra cit.*).

It will now be my endeavour to demonstrate that the anterior ventral ray is not only theoretically (whatever precise significance attaches to this term), but that it has an extremely strong claim to be regarded as actually, a precoracoid.

I shall show that in its position in the girdle, in its relations, and presumably in its genesis, it corresponds essentially to the bone that bears the name precoracoid in extant Amphibia and Reptilia.

In discussing the homology of the several parts composing the shoulder girdle, we should ever keep before us the elementary fact that the primitive shoulder girdle is originally one cartilaginous "*continuum,*" emitting one dorsal ray and one or two ventral rays. The dorsal ray is, by universal consent, known as the scapula; where there is only one ventral ray this is by nearly all comparative anatomists known as coracoid; and where there are two ventral rays, the anterior is designated precoracoid or clavicle. It is con-

* *Italics* mine.

sidered precoracoid by C. Gegenbaur, Fürbringer, Huxley, W. K. Parker, and others, including myself; and is named clavicle by Götte, Hoffmann, and others, their followers.

The chief argument used in my address, from which Professor H. G. Seeley quotes, that this piece in the Plesiosaurian shoulder girdle is really a precoracoid, is the very close agreement noticeable between the Plesiosaurian and Testudinate girdles, a correspondence not merely general, but also of their respective parts; so that, if the anterior ventral ray in Testudinata is a precoracoid, this affords a very strong presumption that the corresponding ray in Plesiosauria is also precoracoid. This presumption appears to me to amount as nearly to proof as the nature of the comparison of these parts admits; but since Professor H. G. Seeley considers "the evidence insufficient to sustain the interpretation," and since, on re-examination, my former conclusion is confirmed, I shall amplify and re-state it.

In comparing skeletons of fossil with those of extant animals, absolutely complete proof of the essential identity of their several parts is, in particular instances, unattainable, and we have to accept *for it presumption* so *strong* as not to allow reasonable doubt. As regards living animals, we have the great aid of embryology to illumine the facts of the mature skeleton; but this valuable aid is, if we except a few very rare instances (*e.g.*, larval, concurrent with adult forms, as in the Batrachial fauna of the Braun-Kohlen formation of Rhenish Prussia), wanting in regard to fossil animal remains, which are so often fragmentary and otherwise imperfect and incomplete.

In Testudinata it is a matter of common elementary knowledge that the primary shoulder girdle (primitively a three-rayed cartilage, having at its centre a hollow for the reception of the caput humeri, the *fossa glenoidalis*) appears, when ossification is completed, to comprise two bones only. Of these, one, the posterior ventral ray, contributes the posterior part of the *fossa glenoidalis*, and sends an expansion inwards towards the mesial line of the body. It is by common consent coracoid, and it requires no further notice.

The other bone has an angulated form, of which one branch ascends to be attached ligamentously at the under surface of the carapace; whilst the other branch bends ventrally inwards, in front of the posterior ray, approaching closely that of the other side, and is mostly attached by its mesial end ligamentously to the entoplastron.

It is respecting the homology of this (anterior) ventral branch that there remains any difference of opinion, since all agree that the dorsally-directed branch is scapula.

As already mentioned, these two branches appear to constitute one bone, which meets the coracoid in the glenoid mass, and with the coracoid forms the glenoid fossa. Of this hollow the apparently

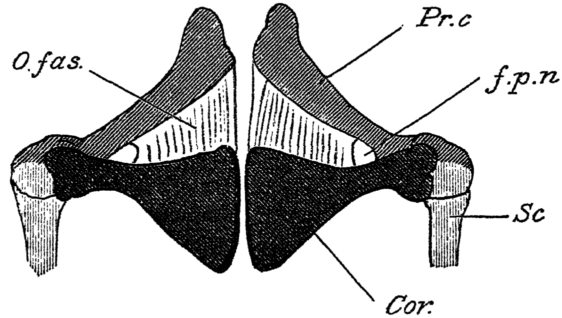


FIG. 9.—Shoulder girdle of *Emys Europæa*. *O. fas.*, obturator fascia; *f.p.n.*, foramen for nerve. From Hoffmann. Bronn's 'Klassen, Rept.,' Bd. 1, Taf. 8, fig. 7.

single, angulated bone forms the upper or the anterior part, the coracoid furnishing the posterior part.

Professor H. G. Seeley says, in his recent paper, "There is no conclusive evidence of the mutual relations of the scapulo-precoracoid to the glenoid cavity in Chelonia." If by this he means that no evidence exists to show whether one only or both of the components of the apparently single bone he thus designates enter into the composition of the glenoid fossa, this statement is, I venture to submit, scarcely justifiable, in view of the embryological investigations of that extremely careful, painstaking observer, H. Rathke, who, writing of the Testudinate shoulder girdle, says: "In the anterior piece (*i.e.*, the united scapula and coracoid), however, I found in the embryo of Chelonia, and in the young of *Chelonia imbricata*, *Trionyx Gangeticus*, and *Terrapene tricarinatus*, that each limb had a particular bony sheath, and that the two sheaths were nowhere confluent, but only at one side of the angle which the two limbs composed they had very closely approached one another; whereas at the outer side of this angle they were still distant from each other, and the entire process projecting from the latter (the angle), and which contained the joint-hollow consisted only of cartilage." "In the young *Sphargis* the two bony sheaths had reached one another at the inner side of the angle formed by the two limbs of the anterior shoulder piece, and had here coalesced, leaving, however, still uncovered the outer side of the angle and the articular process."*

From these observations of H. Rathke, made in different families of *Testudinata*, which are in accord with an observation long previously made by Cuvier in a very young Chelonia, it may very fairly be assumed that in this order generally ossification begins separately

* Rathke, H., 'Ueber die Entwickelung der Schildkröten,' p. 137.

in each of the two branches composing the apparently single bone in the mature girdle, and the apparent oneness of the bone when ossification is complete is due to the coalescence of the two bony masses. Further, as a result of his investigation, H. Rathke knew that the anterior ventral ray enters into the constitution of the glenoid fossa, for, in his discussion of its homology, he says: "Certainly it would be a very peculiar circumstance for Testudinata in connection with this view (viz., that this ray is acromion) that in them the acromion also takes a share in the formation of the hollow of the shoulder joint."*

This places it beyond doubt that H. Rathke found each of the two rays of the "anterior bone" of the Testudinate shoulder girdle entering into the composition of the glenoid fossa.

Now, the separate ossification of each of the branches of the "anterior bone," and the contribution by each to the glenoid fossa, constitutes, I submit, a sufficient warrant for assigning to each equal morphological value, so that if one branch (the dorsal) is scapula, the claim of the other to be precoracoid (clavicular, Götte) cannot be contested.

I repeat, between the Plesiosaurian and the Testudinate girdle there is an extremely close correspondence: each is three-rayed, each has a dorsal ray, each a posterior ventral ray, and each an anterior ventral ray. In Testudinata, each of these three rays contributes to the glenoid fossa; and in the Plesiosaurian girdle no evidence has, so far as I know, been adduced to disprove a similar composition of this fossa.

Without embryological study we had not certainly known whether either, to the exclusion of the other, or both together, of the two branches of the "anterior bone" in Testudinata helped to form the glenoid fossa, since, when ossification is complete, synostosis is so perfect that no trace of earlier separateness remains.

Stannius, indeed, mentions a skeleton of *Emys* in the Berlin Anatomical Museum, in which, on the left side, the anterior ventral limb is suturally united with the ascending limb (scapula). This, however, has been most obligingly re-examined for me by Professor Dr. W. Dames, with the result that he finds the appearance of a suture ambiguous and suggestive rather of an accidental crack, so that this specimen has not the importance previously attached to it.†

In each of the two girdles under consideration the homology of the posterior ventral ray is universally acknowledged to be identic—each is coracoid; and that of the anterior ventral ray cannot, I think,

* Rathke, H., 'Ueber die Entwickelung der Schildkröten,' p. 138.

† I am glad to take this opportunity of expressing my acknowledgment to Professor W. Dames for his assistance kindly rendered me in this matter.—J. W. H.

reasonably be doubted—each is precoracoid (clavicula, Götte). In Testudinata all accept the dorsal ray as representing the blade of the scapula in other Reptilia; but in Plesiosauria the analogous dorsal ray, Professor H. G. Seeley contends, is not an homologous structure. Were it so, then, obviously, the agreement of the two girdles in regard to this element would be seriously compromised.

The ground on which Professor H. G. Seeley rejects the idea that the dorsal process in the Plesiosaurian girdle is homologous with the dorsal ray in the Testudinate girdle, is stated by him in the following passage:—"In Chelonians the ascending process of the scapula extends dorsally towards the vertebræ, while in Sauropterygia it extends backwards above the glenoid articulation for the humerus, and there is no evidence that these structures are homologous" (*cf.* 'Roy. Soc. Proc.,' vol. 51, 1892, p. 122).

It is manifest from this that the backward slant of the Plesiosaurian dorsal scapular process is Professor H. G. Seeley's chief (and only stated) reason for rejecting its homology with the Chelonian shoulder blade. Can this reason be accepted as a sufficient warrant for such rejection? In Birds, does not the long, sword-like shoulder blade slant yet more, bending backwards above and behind the glenoid fossa, in a direction roughly parallel with that of the vertebral column? In his restoration of the Anomodont *Keirognathus cordylus*, has not Professor H. G. Seeley given the shoulder blade a forward inclination so great as to carry its free end so far in advance of the glenoid fossa as to bring it against the centrum of the second vertebra, while a plane laid through the glenoid fossa passes between the seventh and eighth vertebræ? Yet, notwithstanding this excessive forward slant, Professor H. G. Seeley names it scapula. Between these extremes, every degree of slope is observable, so that if the main fact of general dorsal direction above the glenoid fossa be present, this seems to me enough to justify its identification as shoulder blade, and therefore to warrant it and the Chelonian shoulder blade being considered homologous structures.

Should any one still find difficulty in accepting the dorsal *Plesiosaurian* process as shoulder blade, by reason of the somewhat singular position whence it ascends, as homologous with the ascending part of the *Testudinate* scapula, he will find an intermediate step in the Nothosaurian scapula where the corresponding dorsal process* ascends directly from above the glenoid fossa.

In other Nothosauridæ, *e.g.*, in *Lariosaurus*, Cuvier, *Neusticosaurus*, Seeley, to which reference has been already made, the position of the root of the dorsal scapular process corresponds to that in *N. mirabilis*.

Were the part in the Plesiosaurian shoulder girdle, which I call

* The identity of which with that in *Plesiosaurus* has not, so far as I am aware, been doubted.

precoracoid, actually scapula, as Professor H. G. Seeley deems it to be, it must be either *acromion*, or *prescapula*. The former was Conybeare's idea; but Cuvier pointed out this involved an extension of a scapular process mesially inwards towards its fellow of the other side, a construction not known in any Vertebrate; and the same objection lies against a prescapular interpretation.

An objection that might be advanced against homologising the dorsal process in the Plesiosaurian shoulder girdle with the Testudinate shoulder blade, is that it does not ascend so immediately from above the glenoid fossa as does the latter. This is true, but I offer the suggestion (for what it may be worth) that the clue to this difference lies in the dwarfing of this process in *Plesiosaurus;* whilst the growth of the precoracoid has been relatively rapid, with the effect of carrying forward with it the free process of the scapula, which gives to that part of the "anterior bone" somewhat the semblance of a third process. In connection with this it should not be overlooked that in Testudinata before ossification of the shoulder girdle is complete that part of the glenoid mass of cartilage which belongs to the scapula and precoracoid stands off, process-like, from the angle which marks the junction of the two components of the "anterior bone," thus presenting a slight resemblance to that which obtains permanently in *Plesiosauria.*

There remains for discussion the homology of the bony piece or pieces which I have suggested may be omosternal, but which Professor H. G. Seeley contends are clavicles and interclavicle. They lie upon the upper or visceral aspect of the precoracoids (Professor Seeley), therefore within the outer bony frame of the chest; and only to an extent varying in different genera in a ventral view are they visible between the anterior mesial ends of hese bones (precoracoids *mihi*), in advance of the coracoids, above and behind the antero-internal angles of which they are produced backwards to an extent hidden in most skeletons.

In *Plesiosaurus* (type) they form a plate which is more exposed, in a ventral view, than in *Pliosaurus* (R. Owen) in which only a very small part of the omosternum is apparent in the vacuity between the antero-internal angles of the coracoids; whilst in *Colymbosaurus* (H. G. Seeley), the omosternum, if it exist, is completely hidden by the precoracoids (Professor Seeley). Fig. 12, p. 447, 'Geol. Soc. Quart. Journ.,' 1874.

It was this undisputedly deep position of the elements in question, unknown as regards interclavicle and clavicles in any other Vertebrata, that weighed with me against accepting the interclavicular hypothesis held by several previous writers: for I then knew, and I still know, no Vertebrate skeleton in which an interclavicle is thus deeply situated under cover of the other bones of the pectoral frame.

Professor H. G. Seeley certainly has since represented the interclavicle as in part covered by the sternum in *Keirognathus cordylus*,* but I venture to suggest that in the specimen the appearance is not wholly free from ambiguity, and that its damaged condition makes confirmation of so singular a deviation from the usual relation of interclavicle and sternum very desirable.

It was its deep position, together with evidence suggestive of a composite origin, which led me to suggest that this part or parts might be omosternal, since in certain Anoura Professor W. K. Parker had found an omosternum (formed by the fusion of two symmetric halves, each segmented off from the epicoracoid) produced above and behind the anterior border of the precoracoids, and thus occupying a precisely similar position to that occupied by the bone or bones in Plesiosauria. Professor H. G. Seeley's statement that I regarded this bone as indivisible is a misconception, if it is intended to convey that I denied it a dual origin, since in my address I distinctly say, "Some examples show traces of a primitive composition of two similar halves."† I have now before me a drawing of such a Plesiosaurian omosternum (dated June 19, 1869) which I made of a specimen in the Cambridge Museum. In its present form—the postero-external angles are missing—it is an oblong plate, measuring transversely 4·11 ins., and 3 ins. in its antero-posterior extent along the mesial line where are distinct indications of a suture showing its formation by the junction of two halves. That which I regard as the anterior border has the mesial notch seen in some Plesiosauria. The sweep of the posterior border is broken by a projection formed by the backward extension of the postero-internal angle of each of the two halves.

The form of the omosternalia evidently varied in different genera, and it is the opinion of A. Leeds, Esq., who has an excellent knowledge of Enaliosaurian remains, that it varied also with age. An azygos omosternal piece which I saw in his collection at Eyebury, in 1883, was a moderately thin plate, having the outlines of an isosceles triangle, the base of which measured 11·6 cm., and the height 11·3 cm. Its apex was rounded off, and its base slightly incurved. In the direction of a perpendicular from the apex on the base, and also transversely to this direction the figure was slightly curved, which gave a gentle concavity to that surface which I conjectured to be upper.

Two omosternalia, a pair, belonging to another skeleton, had the form of scalene triangles. Of the more perfect of the two the base

* 'Phil. Trans.,' B., 1888, p. 494, Fig. 2. (P. 493, line 3 from bottom, Sternum, the words "so far as to *underlap* the posterior borders of the coracoid," show this "Restoration" to be an inferior, or ventral, view).

† Hulke, J. W., p. 48, 'Proc. Geol. Soc.,' Presidential Address, 1883.

was 6·7 cm. and the height 9 cm. I have not yet found conclusive evidence of the coexistence of such a pair and of an azygos piece in any of the Plesiosaurian remains in the British Museum, and I have thought it probable that the azygos condition was a later stage reached by the fusion of the paired elements as happens in the analogous case of the Anourous omosternum.

Mr. A. Leeds, however, assures me that he possesses one instance of such association of two lateral and a median piece, which he had placed for examination in Professor H. G. Seeley's hands. Such association, if established, would not, however, prove the three pieces to be not *omosternalia* but *clavicular*.

Götte has shown that in Reptilia the interclavicle arises by the coalescence of a piece segmented off from the mesial ends of each clavicle. In Anoura, Parker has shown that the omosternum is formed by the fusion of two pieces segmented off from the anterior extremities of the epicoracoids; in this instance, if a remnant of each lateral segment retained its distinctness, there would be a perfect accord in the principle of the construction of the omosternal and clavicular parts.

By Professor H. G. Seeley the thinness and the surface texture of those pieces is considered decisive of their being originated by the ossification of *membrane* (not of cartilage), an origin which he regards as decisive of their being *clavicular and interclavicular*, and as outweighing the anomaly of their "visceral position." It is possible that they are membrane bones, but this is not yet absolutely certain; whilst their deep position, unknown, if clavicular pieces, in any other Vertebrate skeleton, is not disputed. I submit, then, that the weight of evidence is still in favour of an omosternal homology.

Professor H. G. Seeley argues that these bones cannot be *omosternalia*, because in every existing animal which has an omosternum a sternum also is present, but in no Sauropterygia is there even any trace of a sternum. The usual association of omosternum and sternum may be perfectly true, but since the genesis of these two parts is perfectly distinct, one being a derivative of the epicoracoids, the other a derivative of the costæ, the presence of a sternum is not a necessary antecedent of that of an omosternum. Moreover, though no objective evidence of a sternum in Plesiosauridæ has been preserved, the whole homology of the pectoral girdle, and the high degree of development of the abdominal ribs makes the existence of a cartilaginous sternum a very probable circumstance. Such sternum might not imprint any trace of cartilaginous attachment or other mark of articulation on the postero-internal parts of the coracoids, since their articulation with it might be simply diarthrodial, much as in Lacertilia, in which the border of the coracoid is simply received in a corresponding groove in the sternum.

Professor H. G. Seeley's objection to the importation of the Amphibian plan in explanation of a part of the Plesiosaurian shoulder girdle, of which other parts have been explained by reference to the Chelonian plan of construction, has not, I venture to think, great weight, since, of the early Reptilia, from the time when their remains first began to be studied, it has been a frequent remark that their skeletons comprise structural arrangements which, in existing animals, are now found separately. Moreover, it is thought by some of the ablest comparative anatomists that the Chelonian skeleton shows closer approach to the Amphibian than is to be found elsewhere.

"On Current Curves." By Major R. L. HIPPISLEY, R.E. Communicated by Major MACMAHON, F.R.S. Received May 12,—Read June 2, 1892.

(Abstract.)

1. The object of the present paper is to show how to determine expressions for the current in circuits having iron cores, similar to the well-known equations

$$i = \frac{\mathrm{E}}{\mathrm{R}}(1-e^{-\mathrm{R}t/\mathrm{L}})$$

and

$$i = \frac{\mathrm{E}}{\sqrt{(\mathrm{R}^2+p^2\mathrm{L}^2)}}\sin(pt-\theta)$$

for circuits without iron, which will enable the current curves to be pre-determined by calculation and plotted independently of experiment.

In circuits with iron cores the value of $\frac{d\mathrm{B}}{dt}$ occurring in the original differential equations

$$\mathrm{E}-\frac{d\mathrm{B}}{dt} = \mathrm{R}i \qquad (1),$$

$$\mathrm{E}\sin pt-\frac{d\mathrm{B}}{dt} = \mathrm{R}i \qquad (2)$$

continually alters as i changes. If we could obtain an expression for $\frac{d\mathrm{B}}{dt}$ in terms of i, the substitution of this expression in (1) and (2) should lead us to the required result. But, though such an expression can be found, its substitution will *generally* lead to differential equations which cannot be solved by known methods.

2. In the case represented by (1), where the applied E.M.F. is constant, we can determine by Lagrange's formula of interpolation

the equation to the (B, H) curve of the particular core under consideration. This will be of the form

$$B = a_0 + a_1 H + a_2 H^2 + \ldots + a_n H^n \quad \ldots\ldots\ldots\ldots (3),$$

where n is one less than the number of observed simultaneous values of B and H from which the equation is calculated; whence

$$\frac{dB}{dt} = a_1 L \frac{di}{dt} + 2a_2 L^2 i \frac{di}{dt} + \ldots + na_n L^n i^{n-1} \frac{di}{dt} \quad \ldots\ldots (4),$$

and substituting in the equation

$$E - \frac{dB}{dt} = Ri \quad \ldots\ldots\ldots\ldots\ldots\ldots (5),$$

we get

$$\frac{a_1 L + 2a_2 L^2 i + 3a_3 L^3 i^2 + \ldots + na_n L^n i^{n-1}}{E - Ri} di = dt \quad \ldots\ldots (6),$$

which is easily integrable; and integrating between the proper limits we get after reduction

$$t = \log \frac{E}{E - Ri} \left(\frac{1}{R} a_1 L + \frac{E}{R^2} 2a_2 L^2 + \frac{E^2}{R^3} 3a_3 L^3 + \ldots + \frac{E^{n-1}}{R^n} na_n L^n \right)$$

$$- i \left(\frac{1}{R} 2a_2 L^2 + \frac{E}{R^2} 3a_3 L^3 + \frac{E^2}{R^3} 4a_4 L^4 + \ldots + \frac{E^{n-2}}{R^{n-1}} na_n L^n \right)$$

$$- \tfrac{1}{2} i^2 \left(\frac{1}{R} 3a_3 L^3 + \frac{E}{R^2} 4a_4 L^4 + \frac{E^2}{R^3} 5a_5 L^5 + \ldots + \frac{E^{n-3}}{R^{n-2}} na_n L^n \right)$$

$$- \tfrac{1}{3} i^3 \left(\frac{1}{R} 4a_4 L^4 + \frac{E}{R^2} 5a_5 L^5 + \frac{E^2}{R^3} 6a_6 L^6 + \ldots + \frac{E^{n-4}}{R^{n-3}} na_n L^n \right)$$

$$- \text{\&c., to } n+1 \text{ terms} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (7).$$

The corresponding equation when the E.M.F. is removed and the current is dying away is

$$t = \frac{f_1 L}{R} \log \frac{E}{Ri}$$

$$+ \frac{1}{R} \left(2f_2 L^2 \frac{E}{R} + \tfrac{1}{2} 3f_3 L^3 \frac{E^2}{R^2} + \tfrac{1}{3} 4f_4 L^4 \frac{E^3}{R^3} + \ldots + \frac{1}{n-1} nf_n L^n \frac{E^{n-1}}{R^{n-1}} \right)$$

$$- \frac{1}{R} \left(2f_2 L^2 i + \tfrac{1}{2} 3f_3 L^3 i^2 + \tfrac{1}{3} 4f_4 L^4 i^3 + \ldots + \frac{1}{n-1} nf_n L^n i^{n-1} \right) \quad \ldots (8),$$

the f_1, f_2, f_3, &c., being the coefficients of the powers of H in the equation to the descending (B, H) curve, which is, of course, different to the ascending curve.

3. This method is not applicable to the case in which the impressed E.M.F. is sinusoidal, on account of difficulties of integration. But both cases can be treated in another way :—Take a series of points on the (B, H) curve of the iron core, such that the chords joining them practically coincide with the curve itself. Let B_κ, H_κ and $B_{\kappa+1}$, $H_{\kappa+1}$ be the coordinates of two consecutive points. The equation to the curve between these points is approximately

$$B = m_{\kappa+1}H + \text{constant} \quad \ldots\ldots\ldots\ldots\ldots \quad (9),$$

where
$$m_{\kappa+1} = \frac{B_{\kappa+1}-B_\kappa}{H_{\kappa+1}-H_\kappa},$$

and therefore between these limits

$$\frac{dB}{dt} = m_{\kappa+1} L \frac{di}{dt} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (10).$$

During the time that the current rises from i_κ to $i_{\kappa+1}$, and B and H rise from B_κ and H_κ to $B_{\kappa+1}$ and $H_{\kappa+1}$, and t rises from t_κ to $t_{\kappa+1}$, we have

$$E - m_{\kappa+1} L \frac{di}{dt} = Ri \quad \ldots\ldots\ldots\ldots\ldots \quad (11),$$

and therefore

$$t_{\kappa+1} = t_\kappa + \frac{m_{\kappa+1}L}{R} \log \frac{E-Ri_\kappa}{E-Ri_{\kappa+1}} \quad \ldots\ldots\ldots\ldots \quad (12),$$

which is true to a very close approximation for any simultaneous values of t and i between the above limits. From this equation, since t_0 and i_0 are both zero, we can determine in succession the times at which the current has the known values 0, $\frac{H_1}{L}$, $\frac{H_2}{L}$, ... &c., using that value of m which applies to that particular value of H under consideration. In this way the current curve can be plotted.

On making $E = 0$ in the original differential equation, and observing the proper limits, we get

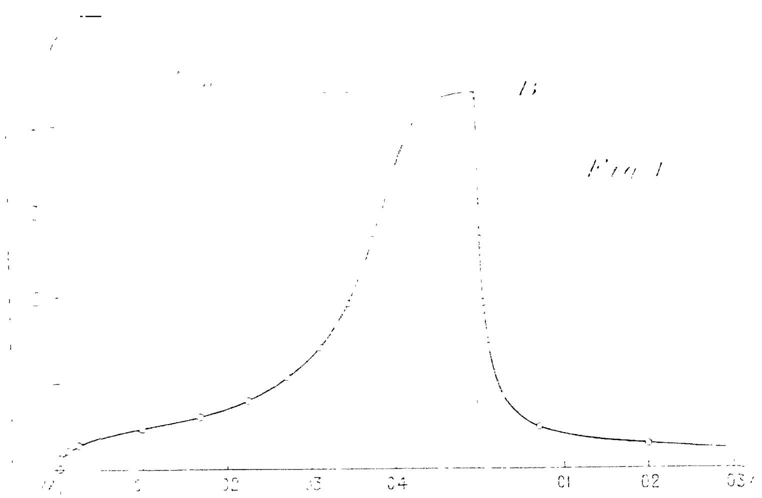
$$t_{n+1} = t_n + \frac{m_{n+1}L}{R} \log \frac{H_n}{H_{n+1}} \quad \ldots\ldots\ldots\ldots \quad (13)$$

as the equation to the curve representing the dying away of the current when the E.M.F. is withdrawn; m_n, m_{n+1} being determined from the descending (B, H) curve.

Fig. 1 and Table I give the results of calculation for a circuit with the following constants :—Resistance, 1 ohm ; E.M.F., 0·4315 volt; self-induction (without iron core), 0·0004 henry.

Table I.

Ascending.		Descending.	
Current.	Time.	Current.	Time.
0·0100	0·00023	0·43150	0·000000
0·0200	0·00095	0·26350	0·000235
0·0270	0·00228	0·19050	0·000768
0·0460	0·01025	0·14050	0·001460
0·0600	0·01649	0·08250	0·003036
0·0825	0·02269	0·04600	0·007670
0·1075	0·02696	0·02700	0·020009
0·1400	0·03100	0·01775	0·039880
0·1905	0·03456	0·00925	0·067370
0·2635	0·03768	0·00475	0·101070
0·3545	0·04048	0·00125	0·185770
0·3750	0·04150	0·00000	∝
0·3875	0·04205		
0·4000	0·04278		
0·4125	0·04367		
0·4315	∝		



4. When the impressed E.M.F. is sinusoidal, we substitute for dB/dt in the equation

$$E \sin pt - \frac{dB}{dt} = Ri \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (14),$$

having determined the various values of dB/dt, as in the foregoing.

As by the present method the value of m changes *abruptly* from

m_κ to $m_{\kappa+1}$, we must employ the *general* solution of (14), which for the interval t_κ, $t_{\kappa+1}$ is

$$i = \frac{\mathrm{E}}{\sqrt{(\mathrm{R}^2 + m_{\kappa+1}{}^2 p^2 \mathrm{L}^2)}} \sin (pt - \theta_{\kappa+1}) + \mathrm{A}_{\kappa+1} e^{-\mathrm{R}t/m_{\kappa+1}\mathrm{L}} \quad (15),$$

in order that the current at the commencement of the interval t_κ, $t_{\kappa+1}$ may have the same value which it had at the end of the interval $t_{\kappa-1} t_\kappa$. The complementary function

$$\mathrm{A}_{\kappa+1} e^{-\mathrm{R}t/m_{\kappa+1}\mathrm{L}}$$

enables us to ensure this condition; for, by taking the constant $\mathrm{A}_{\kappa+1}$ of such a value that equation (15) is satisfied when $i = i_\kappa$ and $t = t_\kappa$, there is no abrupt change in the current. The complementary function, in fact, represents the gradual dying away of whatever excess or defect of current there would be in the circuit when m changes.

Equation (15) is true for all values of i between i_κ and $i_{\kappa+1}$; and, therefore, enables us to find the time $t_{\kappa+1}$ at which the current attains the known value $\mathrm{H}_{\kappa+1}/\mathrm{L}$.

By changing κ into $\kappa + 1$ we obtain similarly the time $t_{\kappa+2}$ at which the current has the value $\mathrm{H}_{\kappa+2}/\mathrm{L}$, and so on.

Thus the determination of $t_{\kappa+1}$ is made to depend upon t_κ, and in order to make a start we must assume that the value of i is known for some definite value of t. When the number of alternations per second is not great, it is not of much consequence what assumption, within reason, is made, as, though the calculated curves will vary with the assumption made, they will all eventually merge into the true periodic current curve at some point which will be exhibited when the *first* evanescence of $\mathrm{A}e^{-\mathrm{R}t/m\mathrm{L}}$ takes place.

As this complementary function is a continually decreasing quantity, it becomes negligible when it is allowed time enough. This opportunity is afforded when the straighter portions of the (B, H) curve are reached, and where the points on the curve can be taken further apart.

When, however, the period of alternation is short in comparison with the time-constant of the circuit, the evanescence of $\mathrm{A}e^{-\mathrm{R}t/m\mathrm{L}}$ does not so readily take place; and it will generally take several cycles before the current shakes down into its truly periodic form. The preliminary assumption ought therefore in such cases to be made with care if it is desired to avoid the labour of calculating the first cycles. But, if the periodicity is quick enough, $\mathrm{A}e^{-\mathrm{R}t/m\mathrm{L}}$ can be taken as a constant, at any rate during the shorter intervals.

Fig. 2 gives the plotted curve calculated for a circuit consisting of 500 turns surrounding an anchor ring, having a coefficient of self-induction (without the core) of 0·0004 henry, and a resistance of

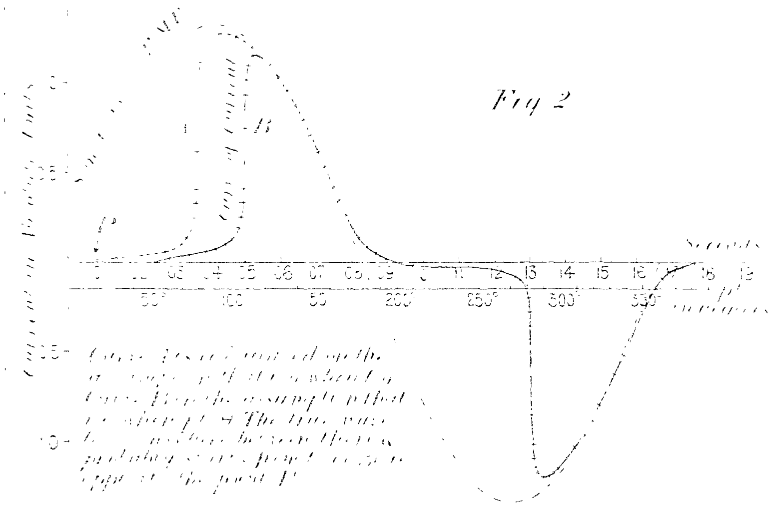
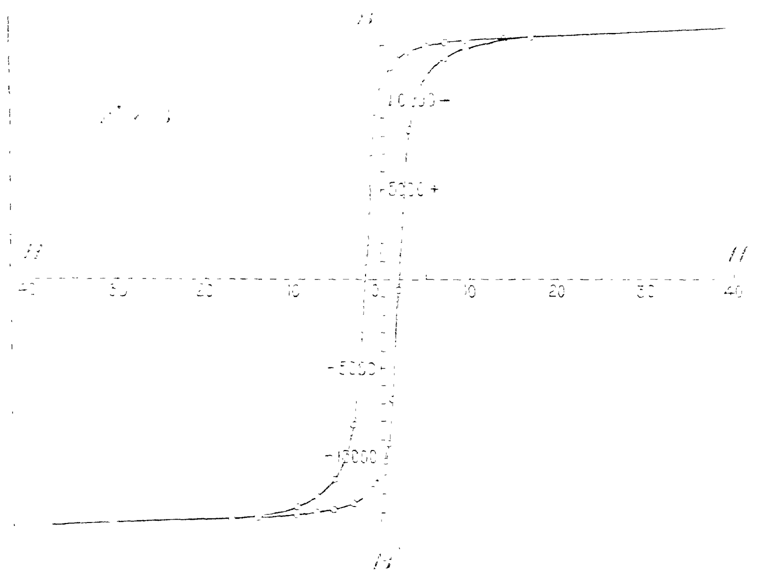


Fig 2
B
P
10
11
12
13
14
15
16
18
19
50°
100
50
200°
250°
300°
350°
0·5
1·0


B
10000
5000
H
H
40
30
20
10
0
10
20
30
40
5000
10000
B'

1 ohm. The impressed E.M.F. is 12·5 volts. The periodicity is slow, being 6 cycles per second, and the true periodic curve is seen to appear before the end of the first half period. Curve A is calculated with the preliminary supposition that $i = 0$ when $t = 0$, and Curve B on the assumption that $i = 0$ when $pt = \theta$, neither of which is strictly correct.

Fig. 3 gives the (B, H) curve, and Table III the observations upon which it is based, of the iron core under consideration.

5. The method of this paper can be applied to the pre-determination of the curves of the primary and secondary currents in transformers and of the curve of magnetic induction with regard to time.

[For, when there is supposed to be no magnetic leakage in the core, the expressions for the primary and secondary currents are—

$$x = \frac{\mathrm{E}\sqrt{(n_1^4\mathrm{S}^2+p^2m^2n_2^4\mathrm{L}^2)}}{\mathrm{D}}\sin(pt-\theta)+\mathrm{A}e^{-qt} \quad \ldots\ldots \quad (16),$$

$$y = -\frac{n_1n_2pm\mathrm{LE}}{\mathrm{D}}\cos(pt-\phi)+\mathrm{B}e^{-qt} \quad \ldots\ldots\ldots\ldots \quad (17),$$

where
$$\tan\theta = \frac{n_1^4pm\mathrm{LS}^2}{n_1^4\mathrm{RS}^2+n_2^2p^2m^2\mathrm{L}^2(n_2^2\mathrm{R}+n_1^2\mathrm{S})},$$

$$\tan\phi = \frac{pm\mathrm{L}(n_2^2\mathrm{R}+n_1^2\mathrm{S})}{n_1^2\mathrm{RS}},$$

$$\mathrm{D} = \sqrt{\{n_1^4\mathrm{R}^2\mathrm{S}^2+p^2m^2\mathrm{L}^2(n_2^2\mathrm{R}+n_1^2\mathrm{S})^2\}}$$

$$q = \frac{n_1^2\mathrm{RS}}{m\mathrm{L}(n_2^2\mathrm{R}+n_1^2\mathrm{S})},$$

R being resistance of primary, S that of secondary, n_1 the number of primary turns, n_2 the number of secondary turns, L the self-induction of the primary.

From (16) and (17) H, the total magnetic force on the core (being $\frac{n_1x+n_2y}{n_1^2}\mathrm{L}$) reduces to

$$\mathrm{H} = \frac{n_1\mathrm{LSE}}{\mathrm{D}}\sin(pt-\phi)+\mathrm{C}e^{-qt} \quad \ldots\ldots\ldots\ldots\ldots \quad (18),$$

where
$$\mathrm{C} = \frac{n_1\mathrm{A}+n_2\mathrm{B}}{n_1^2}\mathrm{L}.$$

Now equation (18) treated in the same way as (15) gives the simultaneous values of H and t, that is, of m and t. These latter substituted in (16) and (17) give the simultaneous values of x and t and y and t.

If there is magnetic leakage it is necessary to discriminate between the H of the primary core and the H of the secondary core; these are respectively—

$$H_1 = \frac{E}{n_1} \cdot \frac{\sqrt{\{L^2S^2+p^2m^2(LN-M^2)^2\}}\sin(pt-\epsilon)}{\sqrt{[\{RS-p^2m^2(LN-M^2)\}^2+p^2m^2(NR+LS)^2]}} + C_1e^{-q_1t}+C_2e^{-q_2t} \ldots\ldots \quad (19),$$

and $$H_2 = \frac{E}{n_2}\frac{MS\sin(pt-\psi)}{\sqrt{[\{RS-p^2m^2(LN-M^2)\}^2+p^2m^2(NR+LS)^2]}} + D_1e^{-q_1t}+D_2e^{-q_2t} \ldots\ldots \quad (20),$$

derived respectively from $H_1 = \frac{1}{n_1}(Lx+My)$ and $H_2 = \frac{1}{n_2}(Mx+Ny)$,

where $$x = \frac{E\sqrt{(S^2+p^2m^2N^2)}\sin(pt-\eta)}{F} + A_1e^{-q_1t}+A_2e^{-q_2t} \ldots\ldots \quad (21),$$

and $$y = -\frac{EpmM\cos(pt-\psi)}{F} + B_1e^{-q_1t}+B_2e^{-q_2t} \ldots\ldots\ldots\ldots \quad (22).$$

In these equations N is the self-induction of the secondary, M is the mutual induction, F is the radicle in the denominator of (19) and (20), while $\tan\eta = pm\frac{LS^2-p^2m^2N(LN-M^2)}{R(S^2+p^2m^2N^2)+p^2m^2M^2S}$,

$$\tan\psi = pm\frac{NR+LS}{RS-p^2m^2(LN-M^2)},$$

$$\tan\epsilon = pm\frac{L^2S^2+RSM^2+p^2m^2(LN-M^2)^2}{R\{LS^2+p^2m^2N(LN-M^2)\}},$$

$$q_1 = \frac{NR+LS-\sqrt{\{(NR-LS)^2+4RSM^2\}}}{2m(LN-M^2)},$$

$$q_2 = \frac{NR+LS+\sqrt{\{(NR-LS)^2+4RSM^2\}}}{2m(LN-M^2)}.$$

September, 1892.]

Table II*a*.—Calculation of First Half Periods, assuming $i = 0$ when $t = 0$.

(The * shows the point at which this coalesces with Table II*b*.)

Ascending.		Descending.		Portion showing evanescence of $Ae^{-Rt/mL}$.			
Current.	Time.	Current.	Time.	$Ae^{-Rt/mL}$.	$\frac{E \sin (pt-\theta)}{\sqrt{(R^2+m^2p^2L^2)}}$.	Current.	Time.
0·00000	0·000000	1·24970	0·042450	0·1175	1·2305	1·1130	0·03782
0·00475	0·002799	0·35475	*0·076483	0·0060	1·2450	1·2390	0·04013
0·00925	0·004837	0·26350	0·078920	0·0040	1·2465	1·2425	0·04060
0·01775	0·008255	0·14050	0·082320	0·0020	1·2480	1·2460	0·04106
0·02700	0·013485	0·08250	0·084340	0·0010	1·2490	1·2480	0·04152
0·04600	0·021850	0·04600	0·087050	0·0003	1·2500	1·2497	0·04245
0·06250	0·027197	0·01775	0·089850	0·0001	1·2495	1·2494	0·04291
0·08250	0·030850	0·00000	0·091840	0 0000	1·2490	1·2490	0·04337
0·10750	0·032550			becomes	1·2480	1·2480	0·04384
0·14050	0·033747			negligible	1·2465	1·2465	0·04430
0·19075	0·035264				1·2450	1·2450	0·04476
0·26350	0·035880				1·2305	1·2305	0·04708
0·35475	0·036060				1·2070	1·2070	0·04939
0·43150	0·036310				1·1770	1·1770	0·05171
1·24970	0·042450		True current wave starts from this point.		1·1330	1·1330	0·05402
					1·0820	1·0820	0·05634
					1·0235	1·0235	0·05865
					0·9570	0·9570	0·06097
					0·8835	0·8835	0·06328
					0·8030	0·8030	0·06560
					0·7165	0·7165	0·06791
					0·6245	0·6245	0·07023
					0·5280	0·5280	0·07254
					0·4275	0·4275	0·07486
					*0·3547	0·3547	0·07648

Table II*b*.—Calculation of First Half Periods, assuming $i = 0$ when $pt = \theta$.

(The * shows the point at which this coalesces with Table II*a*.)

Ascending.		Descending.		Portion showing evanescence of $Ae^{-Rt/mL}$.			
Current.	Time.	Current.	Time.	$Ae^{-Rt/mL}$.	$\frac{E \sin (pt-\theta)}{\sqrt{(R^2+m^2p^2L^2)}}$.	Current.	Time.
0·00000	0·025470	1·13700	0·053100	0·45600	1·1950	0·7390	0·05032
0·00475	0·025640	0·35475	*0·076483	0·25200	1·1885	0·9365	0·05078
0·00925	0·025975	0·26350	0·078920	0·13950	1·1815	0·9420	0·05124
0·01775	0·026670	0·14050	0·082320	0·07700	1·1740	1·0970	0·05171
0·02700	0·029020	0·08250	0·084340	0·04250	1·1665	1·1240	0·05217
0·04600	0·034470	0·04600	0·087050	0·02350	1·1585	1·1350	0·05263
0·06250	0·038800	0·01775	0·089850	0·01300	1·1500	1·1370	0·05310
0·08250	0·042900	0·00000	0·091840	0·00700	1·1415	1·1345	0·05356
0·10750	0·045810			0·00400	1·1330	1·1290	0·05402
0·14050	0·046960			0·00200	1·1235	1·1215	0·05449
0·19075	0·048640			0·00100	1·1135	1·1125	0·05495
0·26350	0·049256			0·00050	1·1035	1·1030	0·05541
0·35475	0·049670		True current wave starts from this point.	0·00030	1·0930	1·0927	0·05587
0·43150	0·049910			0·00000 becomes negligible	1·0820	1·0820	0·05634
1·13700	0·053100				1·0235	1·0285	0·05865
					0·9570	0·9570	0·06097
					0·8835	0·8835	0·06328
					0·8030	0·8030	0·06560
					0·7165	0·7165	0·06791
					0·6245	0·6245	0·07023
					0·5280	0·5280	0·07254
					0·4274	0·4274	0·07486
					0·4068	0·4068	0·07532
					0·3861	0·3861	0·07578
					0·3653	0·3653	0·07624
					*0·3547	0·3547	0·07648

Table IIc.—Calculation of Second Half Period ($i = 0$ when $t = 0{\cdot}09184$).

Ascending.		Descending.		Portion showing evanescence of $Ae^{-Rt/mL}$.			
Current.	Time.	Current.	Time.	$Ae^{-Rt/mL}$.	$\frac{E \sin(pt-\theta)}{\sqrt{(R^2+m^2p^2L^2)}}$.	Current.	Time.
0·00000	0·091840	1·18400	0·133667	0·3750	1·2265	0·8515	0·13089
0·00475	0·092280	0·35475	0·159813	0·2077	1·2222	1·0145	0·13135
0·00925	0·093230	0·26350	0·16225	0·1145	1·2175	1·1030	0·13181
0·01775	0·096400	0·14050	0·16565	0·0635	1·2125	1·1490	0·13227
0·02700	0·112787	0·08250	0·16767	0·0350	1·2070	1·1720	0·13274
0·04600	0·118375	0·04600	0·17038	0·0195	1·2010	1·1815	0·13320
0·06250	0·122690	0·01775	0·17318	0·0110	1·1950	1·1840	0·13366
0·08250	0·125933	0·00000	0·17517	0·0060	1·1885	1·1825	0·13413
0·10750	0·127506		End of first true half period. After this the same values of current recur as at beginning of true wave, at times differing from former ones by 0·8333 sec. or a half period of alternation.	0·0035	1·1815	1·1780	0·13459
0·14050	0·128646			0·0020	1·1770	1·1750	0·13505
0·19075	0·129487			0·0010	1·1665	1·1655	0·13551
0·26350	0·130086			0·0005	1·1585	1·1580	0·13598
0·35475	0·130494			0·0001	1·1496	1·1495	0·13644
0·43150	0·130748			becomes	1·1415	1·1415	0·13690
1·18400	0·133667			negligible	1·1330	1·1330	0·13737
					1·0820	1·0820	0·13967
					1·0235	1·0235	0·14198
					0·9570	0·9570	0·14430
					0·8835	0·8835	0·14661
					0·8030	0·8030	0·14893
					0·7165	0·7165	0·15124
					0·6245	0·6245	0·15356
					0·5280	0·5280	0·15587
					0·4274	0·4274	0·15819
					0·4068	0·4068	0·15865
					0·3861	0·3861	0·15911
					0·3653	0·3653	0·15957
					0·3547	0·3547	0·15981

Table III.—Observations of (B, H) Curve.*

First half cycle.		Second half cycle.		Third half cycle.		Loop.	
H.	B.	H.	B.	H.	B.	H.	B.
		0·00	+10,980	0·00	−10,800	0·00	+10,840
		− 0·05	+10,970	+ 0·05	−10,795	+ 0·05	+10,845
0·00	0	− 0·19	+10,790	+ 0·19	−10,620	+ 0·19	+10,850
+ 0·40	+ 100	− 0·37	+10,460	+ 0·37	−10,268	+ 0·37	+10,860
+ 0·80	+ 400	− 0·71	+ 9,530	+ 0·71	− 9,270	+ 0·71	+10,890
+ 1·08	+ 941	− 1·08	+ 7,360	+ 1·08	− 6,780	+ 1·08	+10,930
+ 1·84	+ 4,088	− 1·84	+ 20	+ 1·84	− 642	+ 1·84	+11,040
+ 3·30	+ 8,684	− 3·30	− 7,840	+ 3·30	+ 8,190	+ 3·30	+11,480
+ 5·62	+11,380	− 5·62	−11,100	+ 5·62	+11,270	+ 5·62	+12,210
+ 7·63	+12,320	− 7·63	−12,080	+ 7·63	+12,170	+ 7·63	+12,650
+10·54	+12,950	−10·54	−12,690	+10·54	+12,770	+10·54	+13,040
+14·19	+13,280	−14·19	−13,030	+14·19	+13,110	+14·19	+13,300
+17·26	+13,450	−17·26	−13,190	+17·26	+13,280	+17·26	+13,440
+14·19	+13,370	−14·19	−13,110	+14·19	+13,220		
+10·54	+13,250	−10·54	−13,000	+10·54	+13,100		
+ 7·63	+13,100	− 7·63	−12,900	+ 7·63	+12,940		
+ 5·62	+12,970	− 5·62	−12,800	+ 5·62	+12,830		
+ 3·30	+12,680	− 3·30	−12,520	+ 3·30	+12,570		
+ 1·84	+12,240	− 1·84	−12,080	+ 1·84	+12,150		
+ 1·08	+11,850	− 1·08	−11,700	+ 1·08	+11,790		
+ 0·71	+11,590	− 0·71	−11,430	+ 0·71	+11,510		
+ 0·37	+11,277	− 0·37	−11,130	+ 0·37	+11,200		
+ 0·19	+11,100	− 0·19	−10,970	+ 0·19	+11,030		
+ 0·05	+11,000	− 0·05	−10,820	+ 0·05	+10,880		
0·00	+10,980	0·00	−10,800	0·00	+10,840		

* Taken from Professor Ewing's "Magnetism in Iron, &c.," 'Phil. Trans.,' 1885, except the first three observations, which have been made to show the iron starting without initial magnetism. H and B are in absolute units per sq. cm.

November 17, 1892.

Sir JOHN EVANS, K.C.B., Vice-President and Treasurer, in the Chair.

Mr. Frank E. Beddard, Professor C. Le Neve Foster, Dr. Hans Gadow, Mr. Francis Gotch, and Professor T. Jeffery Parker (elected 1888) were admitted into the Society.

A List of the Presents received was laid on the table, and thanks ordered for them.

In pursuance of the Statutes, notice of the ensuing Anniversary Meeting was given from the Chair.

Professor W. G. Adams, Professor Rücker, and Professor W. C. Williamson were by ballot elected Auditors of the Treasurer's accounts on the part of the Society.

The following Papers were read:—

I. "On the Characters and Behaviour of the Wandering (Migrating) Cells of the Frog, especially in relation to Micro-organisms." By A. A. KANTHACK, M.R.C.P., M.B., and W. B. HARDY, M.A. Communicated by Professor M. FOSTER, Sec. R.S. Received November 1, 1892.

(Abstract.)

The paper deals with the results of an investigation of the structure and functions of the wandering (migrating)* cells of the Frog. Certain preliminary observations on Mammals and Crustacea are also included.

The results may be summarised as follows:—

The histology of the wandering cells of the Frog is almost identical with that of the wandering cells of *Astacus*. The different cells are very clearly marked off from one another when seen alive or when in preparations. Excluding red blood corpuscles and platelets, which stand on a different footing from all the rest, the following forms are found:—

* This appellation is used in preference to such terms as "leucocyte" or "white corpuscle," since it is more inclusive.

Normal.	I. Cells normally free in the blood and in the lymph.	(*a*) Eosinophile cells; nucleus horse-shoe shaped or lobed; do not ingest particles; are motile unicellular glands.
		(*b*) Hyaline cells, free from specific granulation; nucleus round with central nucleolus. Phagocytic, *i.e.*, they possess the power of ingesting and digesting discrete particles.
	II. Cells very few in number and small in normal lymph. Normally present in the lacunar spaces of areolar tissue.	(*c*) Basophile cells, spherical, with scanty protoplasm when small; angular, rounded or flattened when large; cell substance charged with tiny basophile granules, which give a vivid rose colour with methylene-blue. Large oval or round vesicular nucleus, sometimes containing irregular chromatin mass and filaments.
Abnormal.	III. Large amœboid cells; vacuolate, frequently with ingesta in the vacuoles, multinuclear, very active and phagocytic.	Giant cells formed by fusion of hyaline cells, similar to the large phagocytic cell of *Astacus*.
	IV. Small bodies, either round and quiescent or amœboid.	Nucleated cells budded off from the eosinophile or hyaline cells.
		Non-nucleated bodies produced by breaking up of red corpuscles.

The hyaline cell is less resistant than the eosinophile cell. Rough manipulation causes a rapid bursting up of the cell, thus recalling the hyaline explosive corpuscles of *Astacus*.

We have studied the functions of these cells in relation to their anti-bacillary action (1) by taking samples of lymph from a Frog at varying intervals after the injection of bacilli, &c.; (2) by inoculating hanging drops suspended in moist chambers and kept at different temperatures, the chambers being sufficiently large to afford plenty of oxygen. By the second method we have been able to observe the conflict between cells and bacilli for continuous periods of eight to nine hours. The *same cells and bacilli* have been watched for the whole period.

In the same manner we have also examined the effect of the injection of finely-divided coagulated proteïd (boiled white of egg solution), Indian ink, vermilion, egg albumen, and anthrax spores. At first we used curarised Frogs to obtain lymph, and this led to the discovery that curare produces a profound alteration in the wandering cells.

The phenomena of leucocytosis have also been examined, and we find the following:—

1. Corresponding with the three different kinds of wandering cells found in the blood and lymph, three kinds of leucocytosis may be distinguished, each characterised by the relatively greater increase in number of one particular kind of cell. This may be illustrated by citing the effect of the injection of finely-divided coagulated proteid, which produces a great increase in the number of the hyaline (phagocytic) cells without a correspondingly large increase in the numbers of the other wandering cell forms. Eosinophile leucocytosis, that is, increase in the numbers of the eosinophile cells, occurs with wonderful rapidity after injection of anthrax bacilli or other micro-organisms, and it is then followed by a leucocytosis of the hyaline cells.

2. The leucocytosis, or increase in the number of the cells, is largely due to the proliferation of the cells themselves. Thus eosinophile leucocytosis, followed by hyaline leucocytosis, occurs out of the body in a hanging drop of lymph. Also we have witnessed the division of the cells in a hanging drop. The phenomena classed under the head of chemiotaxis are undoubtedly to be partly explained by the very rapid power of proliferation by fission of the wandering cells.

The behaviour of the cells towards micro-organisms differs according to the nature of the latter. In this abstract we will confine ourselves to the conflict with *Bacillus anthracis*.

The Frog at ordinary temperatures is absolutely immune against anthrax. When lymph is treated with anthrax bacilli the following phenomena are seen, and may be grouped as successive stages:—

Stage I.—The eosinophile cells are strongly attracted to the anthrax. They apply themselves to the chains of bacilli. When contact is absolutely or nearly effected their cell substance shows the following phenomena:—

1. It is profoundly stimulated, and exhibits quick streaming movements. Ordinarily the eosinophile cell is very sluggish.

2. The eosinophile spherules are discharged: those nearest the bacillus fading and dissolving first.

3. If the eosinophile cells are present in sufficient numbers to match the anthrax, in other words, if they are unharmed by the bacilli, they bud off daughter cells, which are at first free from granules. These creep a short way from the point of conflict, and in a short time spherules appear at one end. Later, these daughter cells seek the same or another focus of conflict. Several eosinophile cells will, towards the close of Stage I, and when their numbers have increased, be massed round one chain, and they ultimately fuse, though the endosarc, with its granules, remains distinct. In this way an eosinophile plasmodium is formed, though the fusion is con-

fined to the more mobile peripheral cell substance. Whether the eosinophile cells or the bacilli win the fight depends largely on their relative numbers. The bacillus is only injured near the eosinophile cell; there the contents become rapidly curdled and irregular in appearance, and may be completely dissolved (it should be noted that Leber has shown that pus dissolves copper, and even platinum, and Kanthack has shown that the pus cell is the eosinophile cell). If the bacillary chains are in great number, then there may not be eosinophile cells enough to attack them all, although the eosinophile cell will extend itself to most attenuated lengths in order to be able to attack as great a length of chain as possible. Even where the chain is not directly attacked, the near presence of eosinophile cells profoundly arrests its development.

If the cells win they early recharge themselves with spherules; *but hese are no longer eosinophile—they are amphophile*; that is, they stain with both eosine and methylene-blue, and rather more readily with the latter.

During the later portion of Stage I the eosinophile cells are aggregating and fusing round the chains of bacilli.

This fusion, and the later and more complete fusion of the hyaline cells is a kind of conjugation, the cells ultimately separating.

During Stage I the hyaline cells, *the phagocytes*, remain quiescent, and *are not attracted towards the bacilli*, though *they may take up indifferent matter such as Indian ink.* In the neighbourhood of a healthy bacillus they appear to be paralysed.

Stage II.—Hyaline cells have now increased in numbers, and come to the eosinophile cell masses surrounding a bacillus and *fuse with them.* The eosinophile cells probably lie extended along a chain; the hyaline cells work with one object, namely, to draw the long-drawn-out mass into a ball. To this end a hyaline cell will attach itself by a broad attachment, and then, by means of long filiform pseudopodia stretched towards more distant parts, it will bend the chain up into a close U, rolling the eosinophile cells round itself, and fusing superficially with them. The superficial fusion of eosinophile cells with the hyaline cell produces violent streaming movements. Other hyaline cells come and fuse with the now lobate spherical and opaque mass. The impact of each successive cell acts as a stimulus, causing streaming and pseudopodial movements, which fade away, to be re-awakened by the arrival of a fresh cell.

We have now a lobed mass, curiously opaque, and—to take one particular instance—formed by the fusion of seven eosinophile cells and four hyaline cells. Three eosinophile cells originally attacked the chain. (It will be noted that we retain the term eosinophile cells, though the second formed spherules are at first amphophile.) This fusion may persist for one to two hours.

Stage III.—The cells of the mass commence to regain their individuality and slowly separate. The separation is in two very distinct stages, and when the individual cells are again to be seen the mass is found to consist of a central giant hyaline plasmodium, formed by the very complete fusion of the four hyaline cells, and enclosed by a crust of eosinophile cells. The first stage in the dissolution of the mass is the separation and wandering away of the eosinophile cells, fully charged with the second set of spherules, which have now become truly eosinophile. A very curious appearance is presented as they shred themselves off the central hyaline mass. This plasmodium or giant cell is now seen to be an amœboid body, with several food vacuoles containing ingesta in the form of the remnants of the chain of bacilli. It pushes out on one side long filiform pseudopodia, which resemble those of the Heliozoa in their sluggish, streaming movements, while from the other side project short round pseudopodia.

The hanging drop contains, at this stage, multitudes of these phagocytic plasmodia, with free eosinophile cells and free hyaline and rose-staining cells.

Stage IV.—This is the second stage of the disintegration of the cell masses. The food vacuoles of the plasmodium close up, and the whole structure becomes lobed, taking on the appearance of a heap of hyaline cells, which subsequently separate into the original four cells.

While these stages are in progress the rose-colouring cells are increasing in size and number. They are at first small and spherical, with not very abundant cell substance. Later they become large, angular, and sometimes vacuolate, and their cell substance becomes completely filled with basophile, rose-staining granules.

The activities of the rose-staining cells are, we believe, directed towards the removal of foreign noxious substance in solution in the plasma. We find that if the bacterial poisons accumulate beyond a certain point they paralyse the eosinophile cells, and destroy the hyaline cells. This is prevented, in part at any rate, by the action of the rose-staining cells. We correlate the increase in the granulation of these cells, or, in other words, the increase in the amount of rose-staining substance, with the removal of the bacterial products.

The conflict thus consists of, *first*, the maiming of the bacilli by the eosinophile cells; *secondly*, the removal of the remains of the bacilli by means of the ingestive and digestive activity of the hyaline cells; and, *thirdly*, the removal of dissolved foreign substances by the rose-staining cells. We do not propose to deal at present with the further processes of repair.

Action of Urari.

It induces extensive leucocytosis.

Stage I.—After three hours lymph drawn is found to contain hyaline and *amphophile* cells, the latter in great abundance.

By treating a hanging drop with urari and methylene-blue, we were able to watch the granules of the eosinophile cells slowly undergo a slight decrease in size and stain with the methylene-blue. The granules of the normal cell never stain with methylene-blue.

Stage II, 12 *hours.*—Repair in progress; numerous large cells present charged with ingesta.

Stage III.—The normal eosinophile cell re-appears. Frogs completely recover from urari in a day or two.

Action of Heat.

Frogs are rendered susceptible to anthrax by being warmed. We therefore inoculated hanging drops and watched them on the warm stage.

We found that the first attack of the eosinophile cells was commenced before the temperature had risen, but never carried out, the cells becoming completely paralysed, and showing no movement for five hours. Therefore there was no phagocytosis, *for this can only follow the eosinophile attack.*

Morphology and Comparative Physiology of these Wandering Cell Elements.

We are now able to point to three animal forms, the Frog and Lamprey, types of a complex and highly developed group, and *Astacus*, a complex member of a group containing animals of widely divergent complexity. In all these different forms of wandering cells occur. These we may class as—

Granular eosinophile.	Found free in the body fluids.
Non-granular hyaline.	" " "
Rose-reacting cell, granular.	Wandering cell which is found in the body fluids, but which also inhabits the spaces of connective tissues, though it is not by any means identical with the connective-tissue cell.

Of these diverse forms we see the archetype in the granular, protective, digestive, absorptive, and constructive (for it contributes to form the fat tissue and scar tissue) blood cell of the primitive animal *Daphnia*, and the granulation of this primitive cell is amphophile and rose-staining, as is also the granulation of the ectoderm of *Daphnia*.

The physiological differentiation we can trace when we see that the eosinophile cell has accentuated the glandular and protective character of the primitive cell; while in its attack by direct contact brought about by pseudopodial activity we see the remnant of the direct pseudopodial and ingestive attack of the primitive cell.

The hyaline cell, or permanently free phagocyte, represents the specialisation of the direct pseudopodial ingestive activity of the primitive cell.

While, lastly, the absorptive powers of the primitive cell are represented by the rose-staining cell of the more differentiated animal forms.

II. "Stability and Instability of Viscous Liquids." By A. B. Basset, M.A., F.R.S. Received October 10, 1892.

(Abstract.)

The principal object of this paper is to endeavour to obtain a theoretical explanation of the instability of viscous liquids, which was experimentally studied by Professor Osborne Reynolds.*

The experiment, which perhaps most strikingly illustrates this branch of hydrodynamics, consisted in causing water to flow from a cistern through a long circular tube, and by means of suitable appliances a fine stream of coloured liquid was made to flow down the centre of the tube along with the water. When the velocity was sufficiently small, the coloured stream showed no tendency to mix with the water; but when the velocity was increased, it was found that as soon as it had attained a certain critical value, the coloured stream broke off at a certain point of the tube and began to mix with the water, thus showing that the motion was unstable. It was also found that as the velocity was still further increased the point at which instability commenced gradually moved up the tube towards the end at which the water was flowing in.

Professor Reynolds concluded that the critical velocity W was determined by the equation

$$Wa\rho/\mu < n,$$

where a is the radius of the tube, ρ the density, and μ the viscosity of the liquid, and n a number; but the results of this paper show that this formula is incomplete, inasmuch as it does not take any account of the friction of the liquid against the sides of the tube.

In the first place, if the surface friction is supposed to be zero, so that perfect slipping takes place, the motion is stable for all veloci-

* 'Phil. Trans.,' 1883, p. 935.

ties. If ϵ^{kt} be the time factor of a disturbance of wave-length λ, the value of k is

$$k = -\frac{2\iota\pi W}{\lambda} - \frac{\mu}{\rho a^2}\left(\frac{4\pi^2 a^2}{\lambda^2} + n^2\right) \text{ .... (1)},$$

where n is a root of the equation $J_1(n) = 0$.

Experiment shows that when the velocity is greater than about 6 inches per second, the frictional tangential stress of water in contact with a fixed or moving solid is approximately proportional to the square of the relative velocity. This introduces a constant β, which may be called the coefficient of sliding friction, whose dimensions are $[ML^{-3}]$, and are therefore the same as those of a density. This constant may have any positive real value; $\beta = 0$ corresponding to perfect slipping or zero tangential stress, whilst $\beta = \infty$ corresponds to no slipping, which requires that the velocity of the liquid should be the same as that of the surface with which it is in contact. Owing to the intractable nature of the general equations of motion of a viscous liquid, I have been unable to obtain a complete solution, except on the hypothesis that β is an exceedingly small quantity. This supposition, I fear, does not represent very accurately the actual state of fluids in contact with solid bodies; but, at the same time, the solution clearly shows that the instability observed by Professor Reynolds does not depend upon viscosity alone, but is due to the action of the boundary upon a *viscous* liquid.

To a first approximation, the real part of k is proportional to

$$\frac{Wa\beta}{\mu} - \frac{(n^2+m^2a^2)^2}{4n^2} \text{ .... (2)},$$

where $2\pi/m$ is the wave-length of the disturbance, and n is a root of the equation $J_1(n) = 0$. Since the second term is a number, this shows that the motion will be stable, provided

$$Wa\beta/\mu < \text{a number}.$$

The experiments of Professor Reynolds conclusively show that the critical velocity at which instability commences is proportional to μ/a; and the fact that the theoretical condition of stability turns out to be that Wa/μ, multiplied by a quantity of the same dimensions as a density, should be less than a certain number, appears to be in substantial agreement with his experimental results.

The results of the investigation may be summed up as follows:—

(i.) *The tendency to instability increases as the velocity of the liquid, the radius of the tube, and the coefficient of sliding friction increase; but diminishes as the viscosity increases.*

(ii.) *The tendency to instability increases as the wave-length* $(2\pi/m)$ *of the disturbance increases.*

The remainder of the paper is occupied with the discussion of a variety of problems relating to jets and wave motion.

I find that when a cylindrical jet is moving through the atmosphere, the tendency of the viscosity of the jet is always in the direction of stability. The velocity of the jet does not affect the stability unless the influence of the surrounding air is taken into account; if, however, this is done, it will be found that it gives rise to a term proportional to the product of the density of the air and the square of the velocity of the jet, whose tendency is to render the motion unstable. The tendency of surface tension (as has been previously shown by Lord Rayleigh) is in the direction of stability or instability according as the wave-length of the disturbance is less or greater than the circumference of the jet.

If, in addition, the jet is supposed to be electrified, the condition of stability contains a term proportional to the square of the charge multiplied by a certain number, n. When the ratio of the circumference of the jet to the wave-length is less than 0·6, n is positive, and the electrical term tends to produce stability; but when this ratio is greater than 0·6, n is negative, and the electrical term tends to produce instability. It must, however, be recollected that when the above ratio is greater than unity the tendency of surface tension is to produce stability; but if the influencing body is capable of inducing a sufficiently large charge, the electrical term (when $2\pi a > \lambda$) will neutralize the effect of surface tension and viscosity, and the motion will be unstable.

The well-known calming effect of "pouring oil on troubled waters" has passed into a proverb. The mathematical investigation of this phenomenon is as follows:—The oil spreads over the water so as to form a very thin film; we may therefore suppose that the thickness l of the oil is so small compared with the wave-length that powers of l higher than the first may be neglected. Also, since the viscosity of olive oil in C.G.S. units is about* 3·25, whilst that of water is about 0·014, the former may be treated as a highly viscous liquid, and the latter as a frictionless one.

The result is as follows:—

Let ρ_1, ρ be the densities of the water and oil, T_1 the surface tension between oil and water, T the surface tension between oil and air, μ the viscosity of the oil, and ϵ^{kt} the time factor, then, to a first approximation,

$$k = -\frac{\{g(\rho_1-\rho)+T_1m^2\}(g\rho-Tm^2)l}{4\mu\{g\rho_1-(T-T_1)m^2\}}.$$

For olive oil, $T_1 = 20·56$, $T = 36·9$, so that $T > T_1$; and I find that

* Osborne Reynolds, 'Phil. Trans.,' 1886, p. 171.

the motion will be stable unless the wave-length of the disturbance lies between about 9/11 and 6/5 of a centimetre. This result satisfactorily explains the effect of oil in calming stormy water.

III. "On the Colour of the Leaves of Plants and their Autumnal Changes." By ARTHUR HILL HASSALL, M.D. Lond. Communicated by the Rt. Hon. Professor HUXLEY, F.R.S. Received June 21, 1892.

IV. "Observations on the Earthquake Shocks which occurred in the British Isles and France during the month of August, 1892." By EDWARD HULL, F.R.S., F.G.S., Professor of Geology in the Royal College of Science. Received October 5, 1892.

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Portrait in oils of Albert von Haller, For. Mem. R.S.; Bronze Medal struck in honour of Sir Benjamin Brodie, 1841; and Napoleon's Vaccination Medal, 1804. Sir J. Paget, Bart., F.R.S.

Bronze Medallion cast in honour of William Hamilton, F.R.S.; and Lee Medal of the Numismatic Society of London. H. Montagu, Esq., F.S.A.

November 24, 1892.

Sir JOHN EVANS, K.C.B., Vice-President and Treasurer, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

In pursuance of the Statutes, notice of the ensuing Anniversary Meeting was given from the Chair, and the list of Officers and Council nominated for election was read as follows:—

President.—The Lord Kelvin, D.C.L., LL.D.

Treasurer.—Sir John Evans, K.C.B., D.C.L., LL.D.

Secretaries.— { Professor Michael Foster, M.A., M.D.
The Lord Rayleigh, M.A., D.C.L.

Foreign Secretary.—Sir Archibald Geikie, LL.D.

Other Members of the Council.—Captain William de Wiveleslie Abney, C.B.; Sir Benjamin Baker, K.C.M.G., LL.D.; Professor Isaac Bayley Balfour, M.A.; William Thomas Blanford, F.G.S.; Professor George Carey Foster, B.A.; Richard Tetley Glazebrook, M.A.; Frederick Ducane Godman, F.L.S.; John Hopkinson, D.Sc.; Professor Joseph Norman Lockyer, F.R.A.S.; Professor John Gray McKendrick, M.D.; William Davidson Niven, M.A.; William Henry Perkin, LL.D.; Rev. Professor B. Price, D.D.; the Marquis of Salisbury, K.G., M.A.; Adam Sedgwick, M.A.; Professor William Augustus Tilden, D.Sc.

The following Papers were read:—

I. "Ionic Velocities." By W. C. Dampier Whetham, B.A., Fellow of Trinity College, Cambridge. Communicated by J. J. Thomson, F.R.S. Received October 19, 1892.

(Abstract.)

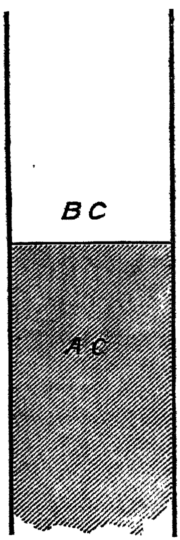
In order to explain the fact that during the electrolysis of a salt solution the ions into which the salt is divided only appear at the electrodes, the intervening solution being unaltered, we must suppose that the ions travel in opposite directions through the liquid. Kohl-

rausch, from the results of a series of experiments on the conductivities of salt solutions, concluded that each ion travelled through dilute solutions with a definite speed when urged forward by a definite potential gradient, independently of the other ion present, and introduced the idea of specific ionic velocity. He calculated the value of this velocity for many substances, using his own conductivity measurements to give the arithmetical sum of the opposite ionic velocities, and Hittorf's "migration" data to give their ratio. From these values of the velocities he worked out the conductivity of many salt solutions, and the agreement with observation of the results so obtained furnished the first confirmation of the theory.

Dr. Oliver Lodge actually observed the velocity of the hydrogen ion as it travelled along a tube containing sodium chloride dissolved in a weak jelly, decolorising phenol-phthalein as it went. He obtained the numbers 0·0029, 0·0026, and 0·0024 cm. per sec. as the velocity of the hydrogen ion under a potential gradient of 1 volt per cm., while Kohlrausch gives 0·0030.

This close agreement led me to undertake a series of experiments in order to find a method of determining ionic velocities which would work under more reliable conditions. Consider the boundary

Fig. 1.

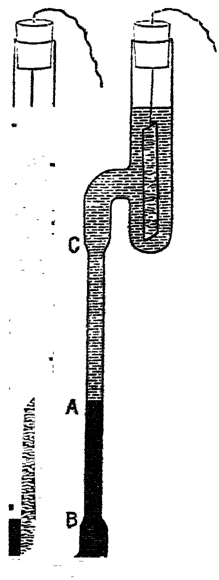


of two salt solutions of slightly different density which have one ion in common, but are of different colours (fig. 1). Let us denote the salts by AC and BC. When a current passes across the boundary

there will be a transference of C ions in one direction and of A and B ions in the other. If A and B are the kations, the colour boundary will move with the current, and its velocity will, in any case, indicate the velocity of the ion causing the change in colour.

The apparatus used (fig. 2) consisted of two vertical glass tubes

FIG. 2.



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about 2 cm. in diameter, joined by a third considerably narrower, which was bent parallel to the others for the greater part of its length. The longer tube was filled with the denser solution to about the level A, and then the lighter solution was run into the other tube from a burette till it just began to trickle over the shoulder and run down to A. The proper solutions were then run into the two limbs at rates just sufficient to keep the junction at rest. The current was passed from platinum electrodes which could be connected with a battery of twenty-six accumulators by means of platinum wires. The corks fitted loosely to allow any gas which might be evolved to escape.

The junction tube had an effective length of 13·8 cm., which, divided into the total difference of potential, gave the potential gradient. The correction due to changes of density produced by the passage of the current can be shown to be quite negligible.

When the solutions are of different specific resistance, there will be a discontinuity of potential gradient at the boundary and a consequent electrification. This can be got rid of entirely by using solutions of the same specific resistance, and in all cases the effect on the velocity of the boundary is non-reversible, and can be approximately eliminated by reversing the current and taking the mean value of the velocity. If the velocity is found to be the same in opposite directions when the current is reversed, the effect must be negligible.

The first solutions used were those of copper and ammonium chlorides dissolved in aqueous ammonia. The copper solution is a deep blue; the other colourless. Their strength was 0·18 gram-equivalent per litre. The mean velocity when the current was passing upward came out 0·0406 cm. per minute, and when passing downwards 0·0441 cm. per minute. In each case the junction moved with the current. The potential gradient was 2·73 volts per cm., which gives as the specific ionic velocity

0·00026 cm. a second.

Kohlrausch gives for infinite dilution

0·00031 cm. a second.

Solutions of potassium permanganate and potassium chloride were taken to show the motion of the acid radicles, and, as it should, the junction moved *against* the current. If we assume that the disappearance of the red colour can only occur where permanganate is replaced by chloride, the motion of the junction can be taken as an indication of the velocity of chlorine. The result for solutions of 0·046 gram-equivalent per litre was 0·00057 cm. per sec., and for solutions of about one-tenth this strength 0·00059 cm. per sec. Kohlrausch gives 0·00053 cm. per sec. as the specific ionic velocity of chlorine.

The success of these preliminary experiments led me to attempt to improve the method. I investigated one pair of salts with exactly the same specific resistance for the same strength, though in cases where this condition is nearly fulfilled (such as that of copper and ammonium chlorides), the error thus introduced can be shown to be negligible.

The direct estimation of the potential gradient is unsatisfactory, but if we measure the specific resistance of the solution (r), the area of the junction tube (A), and the current (γ), we can calculate the specific ionic velocity (v_1) from the observed velocity of the boundary (v), for it is easy to show that

$$v_1 = \frac{v\text{A}}{\gamma r}.$$

This method was used to repeat the copper determination, using solutions whose strength was 0·1 gram-equivalent per litre. The specific resistances of the copper and ammonium chlorides were measured by Fitzpatrick's method, and came out 157×10^9 and 117×10^9 in C.G.S. units respectively. The current was measured by passing it through a previously graduated galvanometer. The velocity of the junction was determined by reading its position at different times by means of a kathetometer.

When the current was passing upwards, the upward velocity was

1·70, 1·60, 1·53, 1·43 cm. per hour; mean, 1·57 cm. per hour,

and when it was passing downwards, the downward velocity was

1·45, 1·65, 1·70 cm. per hour; mean, 1·60 cm. per hour.

This gives a specific ionic velocity in solutions of 0·1 gram-equivalent of

0·000309 cm. per sec.

as compared with Kohlrausch's number for solutions of infinite dilution,

0·00031 cm. per sec.

Solutions of potassium bichromate and potassium carbonate have specific resistances which are very nearly indeed (within 3 per cent.) the same for the same strengths, and a solution of carbonate was adjusted in strength till even this small difference between it and a 0·1 solution of bichromate was much reduced.

The first point I investigated with these solutions was the influence of change of potential gradient on the velocity. These should, on Kohlrausch's theory, obviously be proportional to each other.

At first, all the cells available were used. The velocity of the junction when the current passed downwards was

3·63, 3·39, 3·65, 3·24 cm. per hour; mean, 3·48 cm. per hour

in an upward direction.

When the current was sent upward the velocity was downwards and came out

3·28, 3·55, 3·45 cm. per hour; mean, 3·43 cm. per hour.

This gives $v_1 = 0·00048$ cm. per sec.

An E.M.F. of about one-third that used above was then applied.

Mean downward velocity, 1·44 cm. per hour.
,, upward ,, 1·29 ,, ,,
$v_1 = 0·00047$ cm. per sec.

Thus the value obtained for the specific ionic velocity is independent of the E.M.F. applied, or *the velocity of the ions is proportional to the potential gradient.*

The experiments with the large E.M.F. were repeated with new solutions—

$$v_1 = 0{\cdot}00046 \text{ cm. per sec.}$$

The specific velocity of the bichromate group is not given by Kohlrausch, but can at once be calculated by his method from a knowledge of the molecular conductivity ($9{\cdot}10 \times 10^{-12}$), which was determined by Lenz, and of the migration constant, which was given by Hittorf as 0·502. The velocity comes out

$$v_1 = 0{\cdot}000473,$$

a number identical with the mean value of the measurements described above.

In order to estimate the effect of a discontinuity of potential gradient, another determination of the velocity of this same ion was then made, the carbonate being replaced by potassium chloride, whose conductivity is considerably greater than that of the bichromate ($11{\cdot}13 \times 10^{-12}$ and $9{\cdot}10 \times 10^{-12}$). Two experiments were made:—

(i.) Upward velocity .... Downward velocity .. $\left.\begin{array}{r} v_1 = 0{\cdot}000516 \\ 0{\cdot}000394 \end{array}\right\} v_1 = 0{\cdot}000455.$

(ii.) Upward velocity .... Downward velocity .. $\left.\begin{array}{r} v_1 = 0{\cdot}000483 \\ 0{\cdot}000402 \end{array}\right\} v_1 = 0{\cdot}000443.$

These numbers show that the effect is to increase the velocity in one direction, and to diminish it in the other, while (at all events, if the difference of specific resistance is not great) the mean value gives a fair approximation to that obtained when solutions of identical conductivity are used.

With solutions of different resistances, the junction is often observed to become sharp when travelling in one direction, and vague when travelling in the other. This is owing to the fact that any ion which gets separated from the main body finds itself in a region where the potential gradient is different. Its velocity is therefore altered, and in the first case it rejoins the ranks, and in the second it gets further and further separated from them.

The investigation was also extended to the case of alcoholic solutions. These possess a very much less conductivity than the corresponding aqueous ones, and the question whether Kohlrausch's theory still held good seemed of great interest. No data for the migration constants are known; hence a modification of the method was applied. An experimental measurement of the velocities of both ions of some

salt was made, and their sum compared with the value deduced from the conductivity.

The first salt used was cobalt chloride, the alcoholic solution of which is of a deep blue colour. The velocity of the chlorine ion was measured by setting up the cobalt chloride with cobalt nitrate, the colour of which is red, and that of the cobalt by a cobalt chloride and calcium chloride pair, the latter salt being colourless.

Some little difficulty was encountered in getting solutions of convenient strength. If very weak, the colours were not easily seen, while, if the strength approached 0·1 gram-equivalent per litre, irregularities of behaviour appeared. Finally, solutions of 0·05 gram-equivalent were used, but even here the effects of too great concentration were still appreciable.

Chlorine..........	$v_1 = 0·000026$
Cobalt............	$0·000022$
The sum is........	$U = 0·000048$

This can be calculated from the conductivity ($2·86 \times 10^{-13}$), and comes out

$$U = 0·000060.$$

Cobalt nitrate was then investigated. Its conductivity is greater than that of the chloride ($3·80 \times 10^{-13}$), which leads us to expect that its behaviour will be normal at concentrations greater than those at which the chloride becomes irregular. Such was found to be the case. We should also suppose that its agreement with theory will be closer. The pairs used were cobalt nitrate—cobalt chloride and cobalt nitrate—calcium nitrate.

Nitrate group (NO_3)............	$v_1 = 0·000035$
Cobalt........................	$v_1 = 0·000044$

The sum of these is

$$U = 0·000079.$$

The value calculated from the conductivity is

$$U = 0·000079.$$

The explanation of the irregularities observed in strong solutions is most easily found by the supposition that complex ions are formed as the strength increases. The further discussion of this point is postponed till some more experiments I am making are ready for publication.

A table of results is appended:—

Specific Ionic Velocities.

I. Aqueous Solutions.

Ion.	Velocity observed.	Velocity calculated from Kohlrausch's theory.
Copper	0·00026* 0·000309	0·00031
Chlorine	0·00057* 0·00059*	0·00053
Bichromate group (Cr_2O_7)	0·00048 0·00047 0·00046	0·000473

II. Alcoholic Solutions.

Salt.	Velocity of anion (observed).	Velocity of kation (observed).	Sum of velocities (observed).	Sum of velocities (calculated).
Cobalt chloride	0·000026	0·000022	0·000048	0·000060
Cobalt nitrate	0·000035	0·000044	0·000079	0·000079

II. "Memoir on the Theory of the Compositions of Numbers." By P. A. MacMahon, Major R.A., F.R.S. Received November 17, 1892.

(Abstract.)

In the theory of the partitions of numbers the order of occurrence of the parts is immaterial. Compositions of numbers are merely partitions in which the order of the parts is essential. In the nomenclature I have followed H. J. S. Smith and J. W. L. Glaisher. What are called "unipartite" numbers are such as may be taken to enumerate undistinguished objects. "Multipartite" numbers enumerate objects which are distinguished from one another to any given extent; and the objects are appropriately enumerated by an ordered assemblage of integers, each integer being a unipartite number which specifies the number of objects of a particular kind; and such assemblage constitutes a multipartite number. The 1st

* Preliminary determinations.

Section treats of the compositions of unipartite numbers both analytically and graphically. The subject is of great simplicity, and is only given as a suitable introduction to the more difficult theory, connected with multipartite numbers, which is developed in the succeeding sections.

The investigation arose in an interesting manner. In the theory of the partitions of integers, certain partitions came under view which may be defined as possessing the property of involving a partition of every lower integer in a unique manner. These have been termed "perfect partitions," and it was curious that their enumeration proved to be identical with that of certain expressions which were obviously "compositions" of multipartite numbers.

The 2nd Section gives a purely analytical theory of multipartite numbers.

$$\overline{p_1p_2p_3\ldots p_n}$$

is the notation employed in the case of the general multipartite number of order n. The parts of the partitions and compositions of such a number are themselves multipartite numbers of the same order. Of the number $\overline{21}$ there exist

Partitions.	Compositions.
$(\overline{21})$	$(\overline{21})$
$(\overline{20}\,\overline{01})$	$(\overline{20}\,\overline{01})$, $(\overline{01}\,\overline{20})$
$(\overline{11}\,\overline{01})$	$(\overline{11}\,\overline{10})$, $(\overline{10}\,\overline{11})$
$(\overline{10}^2\,\overline{01})$	$(\overline{10}\,\overline{01})$, $(\overline{10}\,\overline{01}\,\overline{10})$, $(\overline{01}\,\overline{10}^2)$.

The generating function which enumerates the composition has the equivalent forms

$$\frac{h_1+h_2+h_3+\ldots}{1-h_1-h_2-h_3-\ldots},$$

$$\frac{a_1-a_2+a_3-\ldots}{1-2(a_1-a_2-a_3-\ldots)},$$

where h_s, a_s represent respectively the sum of the homogeneous products of order s and the sum of the products s together of quantities

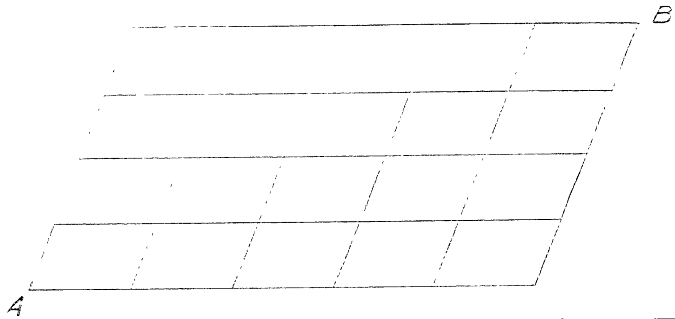
$$\alpha_1, \alpha_2, \alpha_3, \ldots, \alpha_n,$$

and the number of compositions of the multipartite

$$\overline{p_1p_2\ldots p_n}$$

is the coefficient of $\alpha_1^{p_1}\alpha_2^{p_2}\ldots\alpha_n^{p_n}$ in the development according to ascending powers.

Section 3 is taken up with the graphical representation of bipartite numbers. A reticulation is formed which consists of a series of points through each of which straight lines pass in two definite directions, the boundary of the whole being a parallelogram.



The figure AB is the graph of the number $\overline{54}$. A composition of this number is defined by fixing nodes at certain points which possess the property that no point is at once above and to the left of any other point; the parallelogram between adjacent nodes is the graph of a certain number, and in passing through the nodes in succession from A to B an ordered assemblage of numbers is found which constitutes a composition of the number which is represented by the whole graph.

This conception leads to theorems of a new kind which are generalised in Section 4 to include tripartite and multipartite numbers. This section is the most important part of the investigation. It is established that

$$\frac{1}{2}\,\frac{1}{\{1-s_1(2\alpha_1+\alpha_2+\ldots+\alpha_n)\}\,\{1-s_2(2\alpha_1+2\alpha_2+\ldots+\alpha_n)\}\ldots\{1-s_n(2\alpha_1+2\alpha_2+\ldots+2\alpha_n)\}}$$

is also a generating function which enumerates the compositions; the coefficient of

$$s_1^{p_1}s_2^{p_2}\ldots s_n^{p_n}\alpha_1^{p_1}\alpha_2^{p_2}\ldots\alpha_n^{p_n}$$

being the number of compositions possessed by the multipartite

$$\overline{p_1p_2\ldots p_n}.$$

The generating function of the previous section 2 may, by the addition of the fraction $\frac{1}{2}$ and the substitution of $s_1\alpha_1$, $s_2\alpha_2$, &c., for α_1, α_2, &c., be thrown into the form

$$\frac{1}{2}\,\frac{1}{1-2\left(\Sigma s_1\alpha_1-\Sigma s_1s_2\alpha_1\alpha_2+\ldots(-)^{n+1}s_1s_2\ldots s_n\alpha_1\alpha_2\ldots\alpha_n\right)},$$

and hence these two fractions, in regard to the terms in their expansions which are products of powers of $s_1\alpha_1, s_2\alpha_2, \ldots, s_n\alpha_n$, must be identical. This fact is proved by means of the identity—

$$\tfrac{1}{2}\frac{1}{\{1-s_1(2\alpha_1+\alpha_2+\ldots+\alpha_n)\}\{1-s_2(2\alpha_1+2\alpha_2+\ldots+\alpha_n)\}\ldots\{1-s_n(2\alpha_1+2\alpha_2+\ldots+2\alpha_n)\}}$$

$$=\tfrac{1}{2}\frac{1}{1-2(\Sigma s_1\alpha_1-\Sigma s_1s_2\alpha_1\alpha_2+\ldots(-)^{n+1}s_1s_2\ldots s_n\alpha_1\alpha_2\ldots\alpha_n)}$$

multiplied by

$$1+\Sigma\frac{2(\text{A}_{\kappa_1}+\alpha_{\kappa_1})\ldots(\text{A}_{\kappa_t}+\alpha_{\kappa_t})-(\text{A}_{\kappa_1}+2\alpha_{\kappa_1})\ldots(\text{A}_{\kappa_t}+2\alpha_{\kappa_t})}{(1-\text{S}_{\kappa_1})\ldots\ldots\ldots\ldots(1-\text{S}_{\kappa_t})}s_{\kappa_1}s_{\kappa_2}\ldots s_{\kappa_n},$$

where

$$\text{S}_\kappa = s_\kappa(2\alpha_1+\ldots+2\alpha_\kappa+\alpha_{\kappa+1}+\ldots+\alpha_n) = s_\kappa(\text{A}_\kappa+2\alpha_\kappa),$$

and the summation is in regard to every selection of t integers from the series

$$1, 2, 3, \ldots n,$$

and t takes all values from 1 to $n-1$.

This remarkable theorem leads to a crowd of results which are interesting in the theory of numbers. One result in the pure theory of permutations may be stated.

Calling a contact $\alpha_u\alpha_t$ a *major* contact when $u > t$, the number of permutations of the letters in the product

$$\alpha_1^{p_1}\alpha_2^{p_2}\ldots\alpha_n^{p_n}$$

which possess exactly s major contacts is given by the coefficient of

$$\lambda^s\alpha_1^{p_1}\alpha_2^{p_2}\ldots\alpha_n^{p_n}$$

in the product

$$\{\alpha_1+\lambda(\alpha_2+\ldots+\alpha_n)\}^{p_1}\{\alpha_1+\alpha_2+\lambda(\alpha_3+\ldots+\alpha_n)\}^{p_2}\ldots\{\alpha_1+\alpha_2+\ldots+\alpha_n\}^{p_n},$$

and, moreover, is equal to the number of permutations for which

$$r_2+r_3+\ldots+r_n = s,$$

r_t denoting the number of times that the letter α_t occurs in the first

$$p_1+p_2+\ldots+p_{t-1}$$

places of the permutation.

Section 5 gives an extension of the idea of composition and of the foregoing theorems.

The geometrical method of "trees" finds here a place, and, lastly, there is the fundamental algebraic identity—

$$\frac{1}{k}\frac{1}{\{1-s_1(ka_1+a_2+\ldots+a_n)\}\{1-s_2(ka_1+ka_2+\ldots a_n)\}\ldots\{1-s_n(ka_1+ka_2+\ldots+ka_n)\}}$$

$$\frac{1}{k}\frac{1}{1-k\Sigma s_1a_1+k(k-1)\Sigma s_1s_2a_1a_2-\ldots+(-)^nk(k-1)^{n-1}s_1s_2\ldots s_na_1a_2\ldots a_n}$$

multiplied by

$$1+\Sigma\frac{k(\mathrm{A}_{t_1}+at_1)\ldots(\mathrm{A}_{t_n}+at_n)-(\mathrm{A}_{t_1}+kat_1)\ldots(\mathrm{A}_{t_n}+kat_n)}{(k-1)(1-\mathrm{S}_{t_1})(1-\mathrm{S}_{t_2})\ldots(1-\mathrm{S}_{t_n})}s_{t_1}s_{t_2}\ldots s_{t_n},$$

which reduces to that formerly obtained when k is given the special value 2.

Presents, November 24, 1892.

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November 30, 1892.

ANNIVERSARY MEETING.

The LORD KELVIN, D.C.L., LL.D., President, in the Chair.

The Report of the Auditors of the Treasurer's Accounts, on the part of the Society, was presented, by which it appears that the total receipts on the General Account during the past year, including balances carried from the preceding year and £1,300 received from the Meteorological Office for the purchase of Westwood House, amount to £8,180 6*s.* 11*d.*, and that the total receipts on account of Trust Funds, including balances carried from the preceding year, amount to £6,779 11*s.* 4*d.* The total expenditure for the same period, including £1,300 paid for Westwood House, amounts to £7,021 15*s.* 0*d.* on the General Account, and £2,840 19*s.* 1*d.* on account of Trust Funds, leaving a balance on the General Account of £1,125 10*s.* 4*d.* at the bankers', and £33 1*s.* 7*d.* in the hands of the Treasurer, and, on account of Trust Funds, a balance at the bankers' of £3,938 12*s.* 3*d.*

The thanks of the Society were voted to the Treasurer and Auditors.

The Secretary then read the following Lists:—

Fellows deceased since the last Anniversary (Nov. 30, 1891).

Royal.

His Majesty Pedro II, Ex-Emperor of Brazil.

On the Home List.

Adams, John Couch, D.Sc.
Airy, Sir George Biddell, K.C.B.
Aitken, Sir William, M.D.
Bates, Henry Walter, F.L.S.
Bennett, Sir James Risdon, M.D.
Bowman, Sir William, Bart., M.D.
Bramwell, Right Hon. George William Wilsher, Lord, LL.D.
Caird, Right Hon. Sir James, K.C.B.
Calver, Edward Killwick, Capt. R.N.
Clark, Frederick Le Gros, F.R.C.S.
Devonshire, William Cavendish, Duke of, K.G.
Dittmar, William, LL.D.
Grant, Lieut.-Col. James Augustus, C.B.
Grant, Robert, M.A.
Gregory, Right Hon. Sir William Henry, K.C.M.G.
Henry, William Charles, M.D.
Hirst, Thomas Archer, Ph.D.
Hofmann, August Wilhelm von, Ph.D.
Hunt, Thomas Sterry, LL.D.

Knowles, Sir Francis Charles, Bart., M.A.
Paget, Sir George Edward, K.C.B.
Ramsay, Sir Andrew Crombie, LL.D.
Russell, William Henry Leighton, A.B.
Schorlemmer, Carl, LL.D.
Sherbrooke, Robert Lowe, Viscount, G.C.B.
Sutherland, George Granville William Sutherland-Leveson Gower, Duke of, K.G.
Tennyson, Alfred, Lord, D.C.L.
Thomson, James, LL.D.
Wood, John, F.R.C.S.

On the Foreign List.

Kopp, Hermann Franz Moritz.
Kronecker, Leopold.
Quatrefages de Bréau, Jean Louis Armand de.
Stas, Jean Servais.

Change of Name and Title.

Playfair, Sir Lyon, *to* Lord Playfair.
Thomson, Sir William, *to* Lord Kelvin.

Fellows elected since the last Anniversary.

Armstrong, Lieut.-Col. Robert Young, R.E.
Beddard, Frank Evers, M.A.
Devonshire, Spencer Compton Cavendish, Duke of, K.G.
Fleming, Prof. John Ambrose, D.Sc.
Foster, Prof. Clement Le Neve, D.Sc.
Gadow, Hans, M.A., Ph.D.
Giffen, Robert, LL.D.
Gotch, Prof. Francis, M.A., M.R.C.S.
Herdman, Prof. William Abbott, D.Sc.
Herschell, Right Hon. Farrer, Lord, D.C.L.
Hutton, Capt. Frederick Wollaston, F.G.S.
Joly, John, M.A.
Larmor, Joseph, D.Sc.
Miall, Prof. Louis C., F.L.S.
Peach, Benjamin Neve, F.R.S.E.
Pedler, Prof. Alexander, F.I.C.
Waller, Augustus D., M.D.

On the Foreign List.

Kühne, Willy.
Mascart, Éleuthère Élie Nicolas.
Mendeleeff, Dmitri Ivanovitch.
Newton, Hubert Anson.

The President then addressed the Society as follows:—

Since our last Anniversary Meeting, the Royal Society has lost 29 Fellows on the Home List, and 5 Foreign Members, a sadly great number.

Pedro (Dom) II (d'Alcantara), Emperor of Brazil, December 5, 1891.

Ramsay, Sir Andrew Crombie, December 9, 1891, aged 77.
Stas, Jean Servais, December 13, 1891, aged 78.
Bennett, Sir James Risdon, December 14, 1891, aged 82.
Devonshire, William Cavendish, 7th Duke of, December 21, 1891, aged 83.
Russell, William Henry Leighton, December 28, 1891, aged 68.
Kronecker, Leopold, December 29, 1891.
Wood, John, December 29, 1891, aged 66.
Airy, Sir George Biddell, January 2, 1892, aged 90.
Henry, William Charles, January 7, 1892, aged 88.
Quatrefages de Bréau, Jean Louis Armand de, January 12, 1892, aged 81.
Adams, John Couch, January 21, 1892, aged 72.
Paget, Sir George Edward, January 29, 1892, aged 83.
Caird, Right Hon. Sir James, February 9, 1892, aged 76.
Dittmar, William, February 9, 1892, aged 59.
Grant, (Lieut.-Col.) James Augustus, February 11, 1892, aged 65.
Hunt, Thomas Sterry, February 12, 1892, aged 66.
Bates, Henry Walter, February 16, 1892, aged 67.
Hirst, Thomas Archer, February 16, 1892, aged 61.
Kopp, Hermann Franz Moritz, February 20, 1892, aged 75.
Gregory, Right Hon. Sir William Henry, March 6, 1892, aged 75.
Knowles, Sir Francis Charles, Bart., March 19, 1892, aged 90.
Bowman, Sir William, Bart., March 29, 1892, aged 76.
Hofmann, August Wilhelm von, May 5, 1892, aged 74.
Thomson, James, May 8, 1892, aged 71.
Bramwell, George William Wilsher, Lord, May 9, 1892, aged 84.
Aitken, Sir William, June 25, 1892, aged 67.
Schorlemmer, Carl, June 27, 1882, aged 58.
Clark, Frederick Le Gros, July 19, 1892, aged 82.
Sherbrooke, Robert Lowe, Viscount, July 27, 1892, aged 81.
Sutherland, George Granville William Sutherland-Leveson Gower, Duke of, September 22, 1892, aged 64.
Tennyson, Alfred, Lord (Poet Laureate), October 6, 1892, aged 83.
Grant, Professor Robert, October 24, 1892, aged 78.
Calver, (Captain) Edward Killwick, October 28, 1892, aged 79.

Biographical notices will be found in the Proceedings.

The work of continuing the 'Catalogue of Scientific Papers' is being steadily carried on with the resources at the disposal of the Society. The Council and the Catalogue Committee have had under serious consideration the means which should be adopted to make the Catalogue as useful and as complete as possible. The difficulties on financial and other grounds are very great, but the Council is resolved to persevere in this most valuable work.

During the past year, in the mathematical and physical section of the 'Philosophical Transactions,' eighteen papers have been published, and in the biological section, eleven; the two sections together containing a total of 1235 pages of letterpress, and 50 plates. Of the 'Proceedings,' fourteen numbers have been issued, containing 1223 pages, and 20 plates. This unusually large bulk is partly accounted for by the publication in the 'Proceedings' of certain extra matters which the Council deemed likely to interest the Fellows. One part (No. 307), which forms an Appendix to Volume L, contains results of the Revision of the Statutes, to which I alluded in my Anniversary Address last year. It consists of a summary of the Second and Third Charters, and a copy of the Statutes as now revised, followed by an interesting note on the History of the Statutes, which has been drawn up by our Senior Secretary, Professor Michael Foster. In addition to these matters, the same number contains a complete List of the Portraits and Busts at present in the apartments of the Society, compiled by order of the Library Committee, a work which was much needed, as no such list had been made since Weld's Catalogue, printed thirty-two years ago. The new "list" is not a descriptive catalogue, but the names of the painters and donors, and the dates of the gifts, so far as a thorough and somewhat laborious examination of the Council minutes and Journal books has revealed them, are furnished. The List of Portraits is followed by a full descriptive Catalogue of the Medals at present in the possession of the Society, which has been carefully made by our clerk, Mr. James, under the supervision of the Treasurer.

Another extra number of the 'Proceedings' (No. 310) is devoted to a First Report of the Water Research Committee on the Present State of our Knowledge concerning the Bacteriology of Water, by Professors Percy Frankland and Marshall Ward. It contains 96 pages, full of most valuable information regarding the vitality of micro-organisms in drinking water, to which in a large measure the spread of Asiatic cholera, typhoid fever, and other zymotic diseases is now known to be due.

In my Presidential Address of last year, I referred to this Water Committee as having been appointed by the Royal Society, in alliance with the London County Council; and this first instalment of its work seems amply to justify its originators in their expectations of results, most valuable for the public health, from the investigation which has been commenced.

A third extra number (No. 311) contains the Report of the Committee on Colour Vision. This Committee, from the time of its appointment in March, 1890, held over thirty meetings, in course of which it examined more than 500 persons as to their colour vision, and tried various methods and many kinds of apparatus for colour

testing. The report of the results of the whole inquiry contains a large mass of most interesting matter, and the Committee's work ends in a set of practical recommendations, from which we may hope that much benefit will come, in the prevention of inconvenience and disaster liable to be produced by mistake of colour signals, both at sea and on railways.

Mr. Ellis's communication* to the Royal Society of last May, and Professor Grylls Adams' communication† of June, 1891, both on the subject of simultaneous magnetic disturbances found by observations at magnetic observatories in different parts of the world; the award of a Royal medal two years ago to Hertz, for his splendid experimental work on electro-magnetic waves and vibrations; and Professor Schuster's communication‡ to the Royal Society, of June, 1889, on the "Diurnal Variations of Terrestrial Magnetism;" justify me in saying a few words on the present occasion regarding terrestrial magnetic storms, and the hypothesis that they are due to magnetic waves emanating from the sun.

Guided by Maxwell's "electro-magnetic theory of light," and the undulatory theory of propagation of magnetic force which it includes, we might hope to perfectly overcome a fifty years outstanding difficulty in the way of believing the sun to be the direct cause of magnetic storms in the earth, though hitherto every effort in this direction has been disappointing. This difficulty is clearly stated by Professor W. G. Adams, in the following sentences, which I quote from his Report to the British Association of 1881 (p. 469), "On Magnetic Disturbances and Earth Currents":—"Thus we see that the magnetic changes which take place at various points of the earth's surface at the same instant are so large as to be quite comparable with the earth's total magnetic force; and in order that any cause may be a true and sufficient one, it must be capable of producing these changes rapidly."

The primary difficulty, in fact, is to imagine the sun a variable magnet or electro-magnet, powerful enough to produce at the earth's distance changes of magnetic force amounting, in extreme cases, to as much as 1/20 or 1/30, and frequently, in ordinary magnetic storms, to as much as 1/400 of the undisturbed terrestrial magnetic force.

The earth's distance from the sun is 228 times the sun's radius, and the cube of this number is about 12,000,000. Hence, if the sun were, as Gilbert found the earth to be, a globular magnet, and if it were of the same average intensity of magnetisation as the earth, we see, according to the known law of magnetic force at a distance, that the magnetic force due to the sun at the earth's distance from it, in

* 'Roy. Soc. Proc.,' November, 1822, vol. 52, p. 191.

† 'Phil. Trans.,' vol. 183, 1891–92, p. 131.

‡ 'Phil. Trans.,' vol. 180, 1889, p. 467.

any direction, would be only a twelve-millionth of the actual force of terrestrial magnetisation at any point of the earth's surface in a corresponding position relatively to the magnetic axis. Hence the sun must be a magnet* of not much short of 12,000 times the average intensity of the terrestrial magnet (a not absolutely inconceivable supposition, as we shall presently see) to produce, by direct action simply as a magnet, any disturbance of terrestrial magnetic force sensible to the instruments of our magnetic observatories.

Considering probabilities and possibilities as to the history of the earth from its beginning to the present time, I find it unimaginable but that terrestrial magnetism is due to the greatness and the rotation of the earth. If it is true that terrestrial magnetism is a necessary consequence of the magnitude and the rotation of the earth, other bodies comparable in these qualities with the earth, and comparable also with the earth in respect to material and temperature, such as Venus and Mars, must be magnets comparable in strength with the terrestrial magnet, and they must have poles similar to the earth's north and south poles on the north and south sides of their equators, because their directions of rotation, as seen from the north side of the ecliptic, are the same as that of the earth. It seems probable, also, that the sun, because of its great mass and its rotation in the same direction as the earth's rotation, is a magnet with polarities on the north and south sides of its equator, similar to the terrestrial northern and southern magnetic polarities. As the sun's equatorial surface-velocity is nearly four and a half times the earth's, it seems probable that the average solar magnetic moment exceeds the terrestrial considerably more than according to the proportion of bulk. Absolutely ignorant as we are regarding the effect of cold solid rotating bodies such as the earth, or Mars, or Venus, or of hot fluid rotating bodies such as the sun, in straining the circumambient ether, we cannot say that the sun might not be 1000, or 10,000, or 100,000 times as intense a magnet as the earth. It is, therefore, a perfectly proper object for investigation to find whether there is, or is not, any disturbance of terrestrial magnetism, such as might be produced by a constant magnet in the sun's place with its magnetic axis coincident with the sun's axis of rotation. Neglecting for the present the seven degrees of obliquity of the sun's equator, and supposing the axis to be exactly perpendicular to the ecliptic, we have an exceedingly simple case of magnetic action to be considered: a magnetic force perpendicular to the ecliptic at every part of the earth's orbit and varying

* The moon's apparent diameter being always nearly the same as the sun's, the statements of the last four sentences are applicable to the moon as well as to the sun, and are important in connection with speculation as to the cause of the lunar disturbance of terrestrial magnetism, discovered nearly fifty years ago by Kreil and Sabine.

inversely as the cube of the earth's distance from the sun. The components of this force parallel and perpendicular to the earth's axis are, respectively, 0·92 and 0·4 of the whole; of which the former could only be perceived in virtue of the varying distance of the earth from the sun in the course of a year; while the latter would give rise to a daily variation, the same as would be observed if the red ends of terrestrial magnetic needles were attracted towards an ideal star of declination 0° and right ascension 270°. Hence, to discover the disturbances of terrestrial magnetism, if any there are, which are due to direct action of the sun as a magnet, the photographic curves of the three magnetic elements given by each observatory should be analysed for the simple harmonic constituent of annual period and the simple harmonic constituent of period equal to the sidereal day. We thus have two very simple problems, each of which may be treated with great ease separately by a much simplified application of the principles on which Schuster has treated his much more complex subject, according to Gauss' theory as to the external or internal origin of the disturbance, and Professor Horace Lamb's investigation of electric currents induced in the interior of a globe by a varying external magnet. The sidereal diurnal constituent which forms the subject of the second of these simplified problems is smaller, but not much smaller, than the solar diurnal term which, with the solar semi-diurnal, the solar ter-diurnal, and solar quarter-diurnal constituents, form the subjects of Schuster's paper. The conclusion at which he has arrived, that the source of the disturbance is external, is surely an ample reward for the great labour he has bestowed on the investigation hitherto; and I hope he may be induced to undertake the comparatively slight extension of his work which will be required for the separate treatment of the two problems of the sidereal diurnal and the solar annual constituents, and to answer for each the question:—Is the source external or internal?

But even though external be the answer found in each case, we must not from this alone assume that the cause is direct action of the sun as a magnet. The largeness of the solar semi-diurnal, ter-diurnal, and quarter-diurnal constituents found by the harmonic analysis, none of which could be explained by the direct action of the sun as a magnet, demonstrate relatively large action of some other external influence, possibly the electric currents in our atmosphere, which Schuster suggested as a probable cause. The cause, whatever it may be, for the semidiurnal and higher constituents would also probably have a variation in the solar diurnal period on account of the difference of temperature of night and day, and a sidereal and annual period on account of the difference of temperature between winter and summer.

Even if, what does not seem very probable, we are to be led by the

analysis to believe that magnetic force of the sun is directly perceptible here on the earth, we are quite certain that this steady force is vastly less in amount than the abruptly varying force which, from the time of my ancestor in the Presidential Chair, Sir Edward Sabine's, discovery,* forty years ago, of an apparent connexion between sun-spots and terrestrial magnetic storms, we have been almost compelled to attribute to disturbing action of some kind at the sun's surface.

As one of the first evidences of this belief, I may quote the following remarkable sentences from Lord Armstrong's Presidential Address to the British Association at Newcastle, in 1863:—

"The sympathy also which appears to exist between forces operating in the sun and magnetic forces belonging to the earth merits a continuance of that close attention which it has already received from the British Association, and of labours such as General Sabine has, with so much ability and effect, devoted to the elucidation of the subject. I may here notice that most remarkable phenomenon which was seen by independent observers at two different places, on the 1st of September, 1859. A sudden outburst of light, far exceeding the brightness of the sun's surface, was seen to take place, and sweep like a drifting cloud over a portion of the solar face. This was attended with magnetic disturbances of unusual intensity, and with exhibitions of aurora of extraordinary brilliancy. The identical instant at which the effusion of light was observed was recorded by an abrupt and strongly marked deflection in the self-registering instruments at Kew. The phenomenon as seen was probably only part of what actually took place, for the magnetic storm in the midst of which it occurred commenced before, and continued after, the event. If conjecture be allowable in such a case, we may suppose that this remarkable event had some connexion with the means by which the sun's heat is renovated. It is a reasonable supposition that the sun was at that time in the act of receiving a more than usual accession of new energy; and the theory which assigns the maintenance of its power to cosmical matter, plunging into it with that prodigious velocity which gravitation would impress upon it as it approached to actual contact with the solar orb, would afford an explanation of this sudden exhibition of intensified light, in harmony with the knowledge we have now attained, that arrested motion is represented by equivalent heat."

It has certainly been a very tempting hypothesis, that quantities of meteoric matter suddenly falling into the sun is the cause, or one of the causes, of those disturbances to which magnetic storms on the earth are due. We may, indeed, knowing that meteorites do fall into the earth, assume without doubt that much more of them fall, in

* Communication to the Royal Society, March 18, 1852 ('Phil. Trans.,' vol. 162, p. 143).

the same time, into the sun. Astronomical reasons, however, led me long ago to conclude that their quantity annually, or per century, or per thousand years, is much too small to supply the energy given out by the sun in heat and light radiated through space, and led me to adopt unqualifiedly Helmholtz's theory, that work done by gravitation on the shrinking mass is the true source of the sun's heat, as given out at present, and has been so for several hundred thousand years, or several million years. It is just possible, however, that the outburst of brightness described by Lord Armstrong may have been due to an extraordinarily great and sudden falling in of meteoric matter, whether direct from extra-planetary space, or from orbital circulation round the sun. But it seems to me much more probable that it was due to a refreshed brightness produced over a larger area of the surface than usual by brilliantly incandescent fluid rushing up from below, to take the place of matter falling down from the surface, in consequence of being cooled in the regular *régime* of solar radiation. It seems, indeed, very improbable that meteors fall in at any time to the sun in sufficient quantity to produce dynamical disturbances at his surface at all comparable with the gigantic storms actually produced by hot fluid rushing up from below, and spreading out over the sun's surface.

But now let us consider for a moment the work which must be done at the sun to produce a terrestrial magnetic storm. Take, for example, the magnetic storm of June 25, 1885, of which Adams gives particulars in his paper of June, 1891 ('Phil. Trans.,' p. 139 and Pl. 9). We find at eleven places, St. Petersburg, Stonyhurst, Wilhelmshaven, Utrecht, Kew, Vienna, Lisbon, San Fernando, Colaba, Batavia, and Melbourne, the horizontal force increased largely from 2 to 2.10 P.M., and fell at all the places from 2.10 to 3 P.M., with some rough ups and downs in the interval. The storm lasted altogether from about noon to 8 P.M. At St. Petersburg, Stonyhurst, and Wilhelmshaven, the horizontal force was above par by 0·00075, 0·00088, and 0·00090 (C.G.S. in each case) at 2.10 P.M.; and below par by 0·0007, 0·00066, 0·00075 at 3 o'clock. The mean value for all the eleven places was nearly 0·0005 above par at 2h. 10m., and 0·0005 below par at 3h. The photographic curves show changes of somewhat similar amounts following one another very irregularly, but with perfectly simultaneous correspondence at the eleven different stations, through the whole eight hours of the storm. To produce such changes as these by any possible dynamical action within the sun, or in his atmosphere, the agent must have worked at something like 160 million million million million horse-power* (12×10^{35} ergs per sec.), which is about 364 times the total horse-power ($3{\cdot}3 \times 10^{33}$ ergs per sec.) of the solar radiation. Thus, in this eight hours of a

* 1 horse power = $7{\cdot}46 \times 10^{9}$ ergs per second.

not very severe magnetic storm, as much work must have been done by the sun in sending magnetic waves out in all directions through space as he actually does in four months of his regular heat and light. This result, it seems to me, is absolutely conclusive against the supposition that terrestrial magnetic storms are due to magnetic action of the sun; or to any kind of dynamical action taking place within the sun, or in connexion with hurricanes in his atmosphere, or anywhere near the sun outside.

It seems as if we may also be forced to conclude that the supposed connexion between magnetic storms and sun-spots is unreal, and that the seeming agreement between the periods has been a mere coincidence.

We are certainly far from having any reasonable explanation of any of the magnetic phenomena of the earth; whether the fact that the earth is a magnet; that its magnetism changes vastly, as it does from century to century; that it has somewhat regular and periodic annual, solar diurnal, lunar diurnal, and sidereal diurnal variations; and (as marvellous as the secular variation) that it is subject to magnetic storms. The more marvellous, and, for the present inexplicable, all these subjects are, the more exciting becomes the pursuit of investigations which must, sooner or later, reward those who persevere in the work. We have at present two good and sure connexions between magnetic storms and other phenomena: the aurora above, and the earth currents below, are certainly in full working sympathy with magnetic storms. In this respect the latter part of Mr. Ellis's paper is of special interest, and it is to be hoped that the Greenwich observations of earth currents will be brought thoroughly into relation with the theory of Schuster and Lamb, extended, as indeed Professor Schuster promised to extend it, to include not merely the periodic diurnal variations, but the irregular sudden changes of magnetic force taking place within any short time of a magnetic storm.

In my Presidential Address of last year I referred to the action of the International Geodetic Union, on the motion of Professor Foerster, of Berlin, to send an astronomical expedition to Honolulu for the purpose of making a twelve months' series of observations on latitude, corresponding to twelve months' simultaneous observations to be made in European observatories; and I was enabled, through the kindness of Professor Foerster, to announce as a preliminary result, derived from the first three months of the observations, that the latitude had increased during that time by $\frac{1}{3}$ sec. at Berlin, and had decreased at Honolulu by almost exactly the same amount. The proposed year's observations, begun in Honolulu on the 1st of June, 1891, were completed by Dr. Marcuse, and an elaborate reduction of them by the permanent Committee of the International Geodetic Union was published a month ago at Berlin. The results are in

splendid agreement with those of the European observatories: Berlin, Prag, and Strasbourg. They prove beyond all question that between May, 1891, and June, 1892, the latitude of each of the three European observatories was a maximum, and of Honolulu a minimum, in the beginning of October, 1891: that the latitude of the European observatories was a minimum, and of Honolulu a maximum, near the beginning of May, 1892: and that the variations during the year followed somewhat approximately, simple harmonic law as if for a period of 385 days, with range of about $\frac{1}{4}$ sec. above and below the mean latitude in each case. This is just what would result from motion of the north and south polar ends of the earth's instantaneous axis of rotation, in circles on the earth's surface of 7·5 metres radius, at the rate of once round in 385 days.

Sometime previously it had been found by Mr. S. C. Chandler that the irregular variations of latitude which had been discovered in different observatories during the last 15 years seemed to follow a period of about 427 days, instead of the 306 days given by Peters' and Maxwell's dynamical theory, on the supposition of the earth being wholly a rigid body. And now, the German observations, although not giving so long a period as Chandler's, quite confirm the result that, whatever approximation to following a period there is, in the variations of latitude, it is a period largely exceeding the old estimate of 306 days.

Newcomb, in a letter which I received from him last December, gave, what seems to me to be, undoubtedly, the true explanation of this apparent discrepance from dynamical theory, attributing it to elastic yielding of the earth as a whole. He added a suggestion, specially interesting to myself, that investigation of periodic variations of latitude may prove to be the best means of determining approximately the rigidity of the earth. As it is, we have now, for the first time, what seems to be a quite decisive demonstration of elastic yielding in the earth as a whole, under the influence of a deforming force, whether of centrifugal force round a varying axis, as in the present case, or of tide-generating influences of the sun and moon with reference to which I first raised the question of elastic yielding of the earth's material many years ago.

The present year's great advance in geological dynamics forms the subject of a contribution by Newcomb to the 'Monthly Notices of the Royal Astronomical Society,' of last March. In a later paper, published in the 'Astronomische Nachrichten,' he examines records of many observatories, both of Europe and America, from 1865 to the present time, and finds decisive evidence that from 1865 to 1890 the variations of latitude were much less than they have been during the past year, and seeming to show that an augmentation took place, somewhat suddenly, about the year 1890.

When we consider how much water falls on Europe and Asia during a month or two of rainy season, and how many weeks or months must pass before it gets to the sea, and where it has been in the interval, and what has become of the air from which it fell, we need not wonder that the distance of the earth's axis of equilibrium of centrifugal force from the instantaneous axis of rotation should often vary* by 5 or 10 metres in the course of a few weeks or months. We can scarcely expect, indeed, that the variation found by the International Geodetic Union during the year beginning June, 1891, should recur periodically for even as much as one or two or three times of the seeming period of 385 days.

One of the most important scientific events of the past year has been Barnard's discovery, on the 9th of September, of a new satellite to Jupiter. On account of the extreme faintness of the object, it has not been observed anywhere except at the Lick Observatory in California. There, at an elevation of 4500 ft., with an atmosphere of great purity, and with a superb refractor of 36″ aperture, they have advantages not obtainable elsewhere. The new satellite is about 112,000 miles distant from Jupiter, and its periodic time is about 11 h. 50 m. Mr. Barnard concludes a short statement of his discovery with the following sentences:—"It will thus be seen that this new satellite makes two revolutions in one day, and that its periodic time about the planet is less than two hours longer than the axial rotation of Jupiter. Excepting the inner satellite of Mars, it is the most rapidly revolving satellite known. When sufficient observations have been obtained, it will afford a new and independent determination of the mass of Jupiter. Of course, from what I have said in reference to the difficulty of seeing the new satellite, it will be apparent that the most powerful telescopes of the world only will show it" (dated Mount Hamilton, September 21, 1892).

Sir Robert Ball, in calling my attention to it, remarks that "it is by far the most striking addition to the solar system since the discovery of the satellites to Mars in 1877." To all of us it is most interesting that during this year, when we are all sympathising with the University of Padua in its celebration of the third centenary of its acquisition of Galileo as a Professor, we have first gained the knowledge of a fifth satellite in addition to the four discovered by Galileo.

The President then presented the Medals awarded by the Society as follows:—

* See 'Brit. Assoc. Reports,' 1876, Address to Section A, pp. 10, 11.

COPLEY MEDAL.

Rudolph Virchow.

Professor Virchow's eminent services to science are known throughout the world, and they are far too varied and numerous for enumeration.

He survives Schwann, Henle, and the other pioneers in several branches of natural history who came from the school of Johannes Müller, and at the present time occupies a position of influence and honour equal to that of his great contemporaries Helmholtz, Ludwig, and Du Bois-Reymond.

His contributions to the study of morbid anatomy have thrown light upon the diseases of every part of the body,* but the broad and philosophical view he has taken of the processes of pathology has done more than his most brilliant observations to make the science of disease.

In histology he has the chief merit of the classification into epithelial organs, connective tissues, and the higher and more specialised muscle and nerve. He also demonstrated the presence of neuroglia in the brain and spinal cord, and discovered crystalline hæmatoidine, and the true structure of the umbilical cord.

In pathology, strictly so called, his two great achievements—the detection of the cellular activity which lies at the bottom of all morbid as well as normal physiological processes, and the classification of the important group of new growths on a natural histological basis—have each of them not only made an epoch in medicine, but have been the occasion of fresh extension of science by other labourers.

In ethnological and archæological science Professor Virchow has made observations which only the greatness of his other work has thrown into the shade; and, so far from confining himself to technical labours, he has been known since he migrated to Würzburg and returned to Berlin as a public-spirited, far-seeing, and enlightened politician.†

Universally honoured and personally esteemed by most of the leading pathologists in this country, as well as on the Continent and in America, who had the good fortune to be his pupils, Professor Virchow is a worthy successor of the many illustrious men of science to whom the Copley Medal has been awarded.

* Among these may be mentioned his discovery of leucæmia, of lardaceous degeneration, and glioma; his reconstruction of the kind of tumour known as sarcoma, and his establishment of the important group of granulomata.

† A short pamphlet, "Ueber die Nationale Bedeutung der Naturwissenschaften," may be mentioned as characteristic of the patriotism, the fairness, and the broad judgment of the author.

Rumford Medal.

Nils C. Dunér, Director of the Observatory of Lund.

Dr. Dunér has been continuously at work, since 1871, at astronomical observations (see 'R.S. Catalogue').

He began to turn his attention to spectroscopic subjects in 1878, and commenced the publication of his systematic work on Stellar Spectra in 1882.

In 1884 he brought to a conclusion his wonderful observations of stars of Vogel's III Class. His memoir contains a detailed study of the spectra of nearly 400 stars, all which are the most difficult objects to observe. This volume is one of the foundations on which any future work in this direction must be based.

In 1891 he published another series of researches on the rotation of the sun, comparing true solar with telluric lines for regions up to 75° of solar latitude. The result showed a diminution of angular velocity with increasing latitude, thus spectroscopically confirming Carrington's results.

Royal Medal.

Professor Charles Pritchard, D.D., F.R.S., Director of the Oxford University Observatory.

Professor Pritchard began his publications on astronomical subjects in 1852. His first paper and several others which have followed, have dealt with the construction of object glasses and telescope adjustments.

He was President of the Royal Astronomical Society in the years 1867 and 1868.

He was appointed first Director of the newly-founded observatory at Oxford in 1874. It is now the most active University observatory in the kingdom, as many as fifteen students receiving instruction in observatory work at times. The services he has rendered to astronomy in devising, and keeping at a high standard, the work of the observatory in many directions, including its use as a school, are very noteworthy.

Immediately on the establishment of the observatory he saw the beneficial effects of photographic investigation, and first applied the method, with the old wet-plate photography, to the problem of the physical libration of the moon. He saw that this problem was encumbered in heliometric work by the fact that a set of the observations must take a considerable time, and therefore they were made on a constantly changing disc, necessitating great labour in reduction. By the observations being made in two or three seconds, the picture of the moon did not alter in the time. The result was to

show important variations from Bouvard's work, which variations in their important particulars were confirmed by Dr. Hartwig.

Next (1885) the relative motions of the Pleiades were taken up with a view of tracing gravitational effects in the various members of the group. This question is not ripe for solving, but it induced heliometer observers to take up the question, and important progress is now being made.

The photometric work detailed in the 'Uranometria Nova Oxoniensis,' also published in 1885, consisted in measuring the light received from all stars visible to the naked eye, to 10° south declination, by means of a wedge photometer devised by Professor Pritchard—a form of photometer now in the hands of many astronomers. In the course of this work Professor Pritchard, at his own expense, took an assistant to Egypt to determine the effects of atmospheric absorption in a more constant climate than that of Oxford. This photometric work has been recognised by the award of the Gold Medal of the Royal Astronomical Society.

Having fully determined the capacity of photography for accurate measurement, Professor Pritchard next applied it to parallax determinations of stars of the second magnitude. Some thirty stars altogether have been investigated, and this work is now in the press. Thirty is a greater number than any other astronomer has attempted.

Professor Pritchard is now working on the International Chart of the Heavens, and taking part in researches to ensure an accurate photometric scale.

Royal Medal.

John Newport Langley, F.R.S.

Some of the most important of Mr. Langley's researches have been upon the Physiology and Histology of Secreting Glands. Extending the observations of Kühne and Lea on the pancreas, Mr. Langley showed in an elaborate series of researches, extending over the salivary and most of the important secreting glands of the body, that the formation, as a morphological element within the secreting cell, at the expense of its protoplasm, of the material to be used in the secretion is a general function of secreting cells. The dependence of this function upon the activity of nerves, and upon other forms of excitation, such as the action of drugs, has been greatly elucidated in the course of these researches. Concurrently with the morphological changes within the cells, the chemical changes which occur within the secretion as the result of nerve activity or inactivity have been investigated, and many important facts brought to light regarding the nature of the action or modifications of the action which may be brought to bear upon the secreting cell through the nervous system.

These researches are published partly in the 'Philosophical Transactions,' and partly in a long series of articles in the 'Journal of Physiology,' which have extended over several years. It is not too much to say that these researches of Mr. Langley upon secreting glands give him a claim to occupy the highest rank as a physiological investigator.

The other most important researches which Mr. Langley has published have been—(1.) Upon the central nervous system, including especially an investigation into the anatomical changes which result from central lesions; (2.) Upon the sympathetic nervous system, and particularly a number of researches, based upon physiological methods, into its peripheral distribution to involuntary muscle and glands. Mr. Langley's eminence in those branches of physiology to which he has mainly devoted his attention is universally admitted, and has been publicly recognised by his having been requested more than once by international assemblies of physiologists to investigate and report on difficult cases submitted to them (*vide* 'Transactions of the International Medical Congress,' 1881, and 'Proceedings of the Physiological Congress at Basel,' 1890).

Davy Medal.

François Marie Raoult, of Grenoble, Correspondent of the Academy of Sciences.

The accounts of Professor Raoult's researches on the freezing points of solutions, and on the vapour pressures of solutions, form a long series of papers which have appeared from time to time in the 'Comptes Rendus' and 'Annales de Chimie,' from 1871 down to the present time. Our previous knowledge of these subjects was only fragmentary and disjointed, but he has placed it on a new footing, and established general laws relating to the depression of the freezing points and lowering of the vapour pressures of liquids holding other substances in solution. These laws are of great importance, both to chemistry and physics. Their validity is not disputed, and, while theories of solution are much discussed, it is acknowledged that no theory can stand which does not satisfy the conditions which Raoult, by an induction from a very large number of observations on a great variety of substances, has shown to be the order of nature.

Darwin Medal.

Sir Joseph Dalton Hooker, F.R.S.

Although the regulations relating to the award of this medal direct that it is to be treated rather as a means of encouraging young naturalists to fresh exertion than as a reward for the life-long labours

of the veteran, there would seem to be a special appropriateness in awarding it to one who was intimately associated with Mr. Darwin in the preparation of the 'Origin of Species.' That no one was more closely associated than Sir J. D. Hooker with Mr. Darwin in the work is abundantly proved by the following passage in the introduction to the 'Origin of Species':—"I cannot, however, let this opportunity pass without expressing my deep obligations to Dr. Hooker, who, for the last fifteen years (1844–59), has aided me in every possible way by his large stores of knowledge and his excellent judgment."

The Statutes relating to the election of Council and Officers were then read, and Sir Erasmus Ommanney and Mr. Symons, having been, with the consent of the Society, nominated Scrutators, the votes of the Fellows present were taken, and the following were declared duly elected as Council and Officers for the ensuing year:—

President.—The Lord Kelvin, D.C.L., LL.D.

Treasurer.—Sir John Evans, K.C.B., D.C.L., LL.D.

Secretaries.— { Professor Michael Foster, M.A., M.D.
The Lord Rayleigh, M.A., D.C.L.

Foreign Secretary.—Sir Archibald Geikie, LL.D.

Other Members of the Council.

Captain William de Wiveleslie Abney, C.B.; Sir Benjamin Baker, K.C.M.G., LL.D.; Professor Isaac Bayley Balfour, M.A.; William Thomas Blanford, F.G.S.; Professor George Carey Foster, B.A.; Richard Tetley Glazebrook, M.A.; Frederick Ducane Godman, F.L.S.; John Hopkinson, D.Sc.; Professor Joseph Norman Lockyer, F.R.A.S.; Professor John Gray McKendrick, M.D.; William Davidson Niven, M.A.; William Henry Perkin, LL.D.; Rev. Professor B. Price, D.D.; the Marquis of Salisbury, K.G., M.A.; Adam Sedgwick, M.A.; Professor William Augustus Tilden, D.Sc.

The thanks of the Society were given to the Scrutators.

Balance Sheet. 1892.

Statement of Receipts and Expenditure from November 12th, 1891, to November 12th, 1892.

	£	s.	d.	£	s.	d.
To Balance at Bank, 12th November, 1891				1,022	5	8
,, Balance in hand, Catalogue Account	4	14	1	19	11	10
,, ,, Petty Cash	14	17	9			
,, Compositions				240	0	0
,, Admission Fees				20	0	0
,, Annual Contributions, at £4	£532	0	0	1,036	0	0
,, at £3	504	0	0			
,, Fee Reduction Fund, in lieu of Admission Fees and Annual Contributions				320	0	0
,, Rents:						
Fee Farm, Lewes	18	14	5	116	4	5
Mablethorpe Estate	97	10	0			
,, Ground Rents				604	10	6
,, Dividends (exclusive of Trust Funds)				2,018	2	5
,, Interest on Mortgage Loan (Duke of Norfolk)				109	4	0
,, Sale of Transactions and Proceedings				698	4	11
,, Sale of Catalogue				34	15	11
,, Sale of Krakatoa Report (leaving £72 5*s.*, 2*d.* Expenditure in excess of Receipts)				19	7	1
,, Transfer from Handley Fund on account of Catalogue				187	2	4
,, Sale of Lendenfeld Monograph (leaving £664 19*s.* 1*d.* expenditure in excess of receipts)				13	7	8
,, Interest on Bank Deposit Account				71	10	2
,, Meteorological Office, for purchase of Westwood House				1,300	0	0
,, Water Research Grants (leaving £150 further now due from London County Council)				350	0	0
				£8,180	6	11

	£	s.	d.	£	s.	d.
By Salaries, Wages, and Pension				1,650	3	10
,, Catalogue of Scientific Papers				387	4	0
,, Books for the Library				218	15	10
,, Printing and Advertising Transactions, and Separate Copies to Authors and Publisher	291	9	8	2,184	6	10
,, Ditto Proceedings, Nos. 303 to 315	570	16	9			
,, Ditto Miscellaneous	111	17	9			
,, Paper for Transactions and Proceedings	384	16	3			
,, Binding ditto	45	6	2			
,, Engraving and Lithography	780	0	3			
,, Soirée and Anniversary Expenses				174	16	7
,, Coal, Lighting, &c.	221	9	9	650	0	1
,, Office Expenses	67	13	3			
,, House Expenses	183	17	11			
,, Tea Expenses	15	3	11			
,, Fire Insurance	55	5	0			
,, Taxes	45	12	6			
,, Advertising Meetings	18	13	6			
,, Postage, Parcels, and Petty Charges	28	10	5			
,, Miscellaneous Expenses	13	13	10			
,, Law Charges				41	7	10
,, Carrington Donation				30	0	0
,, Westwood House, Purchase				1,300	0	0
,, Water Research, Payments				385	0	0
,, Balance at Bankers				1,125	10	4
Including £500 "Challenger" Account and £200 Catalogue Account.						
,, Balance on hand, Catalogue Account	19	4	1	33	1	7
,, Ditto, Petty Cash	13	17	6			
				£8,180	6	11

Trust Funds.

	£	s.	d.	£	s.	d.
To Balance at Bank 12th November, 1891:—						
General Account	1,897	4	6			
Fee Reduction Fund Account	204	15	1	3,756	2	11
Scientific Relief Fund Account	1,450	11	5			
Joule Memorial Fund Account	203	11	11			
,, Scientific Relief Fund, Dividends, and Donations	657	12	11			
,, Donation Fund, Dividends, and Transfer from Jodrell Fund	389	11	6			
,, Rumford Fund, Dividends	62	9	8			
,, Bakerian and Copley Medal Fund, Dividends, &c.	54	10	0			
,, Keck Bequest, Dividends	23	8	0			
,, Wintringham Fund, Dividends, &c.	32	4	0			
,, Davy Medal Fund, Dividends, &c.	32	3	6	3,023	8	5
,, Gassiot Trust, Dividends and Bonds drawn	850	6	3			
,, Handley Fund, Dividends	187	2	4			
,, Jodrell Fund, Dividends	138	19	4			
,, Fee Reduction Fund, Dividends	423	3	0			
,, Darwin Memorial Fund, Dividends	85	16	0			
,, Joule Memorial Fund, Dividends	42	12	2			
,, Brady Library Account, Interest	3	9	9			
,, Gunning Fund, Interest	40	0	0			
				£6,779	11	4

	£	s.	d.	£	s.	d.
By Scientific Relief Fund, Grants	575	0	0			
,, Donation Fund, Grants	519	13	4			
,, Bakerian and Copley Medal Fund, Medal, and Lecture	8	12	0			
,, Keck Bequest, Payment to Foreign Secretary	23	8	0			
,, Wintringham Fund, Payment to Foundling Hospital	32	4	0			
,, Davy Medal Fund, Gold Medals	32	8	9	2,840	19	1
,, Gassiot Trust, Payments to Kew Committee and Investment	822	8	0			
,, Handley Fund, Transfer to General Account, for Catalogue	187	2	4			
,, Jodrell Fund, Transfer to Donation Fund	138	19	4			
,, Fee Reduction Fund, Transfer to Royal Society General Account	320	0	0			
,, Joule Memorial Fund, Tablet and Abbey Fees	181	3	4			
,, Balance at Bankers:—						
General Account	2,032	9	1			
Fee Reduction Fund Account	307	18	1	3,938	12	3
Scientific Relief Fund Account	1,533	4	4			
Joule Memorial Fund Account	65	0	9			
				£6,779	11	4

Estates and Property of the Royal Society, including Trust Funds.

Estate at Mablethorpe, Lincolnshire (55A. 2R. 2P.), rent £85 per annum.

Ground Rent of House, No. 57, Basinghall Street, rent £380 per annum.

" " of 23 houses in Wharton Road, West Kensington, rents £253 per annum.

Fee Farm Rent, near Lewes, Sussex, £19 4*s.* per annum.

One-fifth of the clear rent of an estate at Lambeth Hill, from the College of Physicians, about £52 per annum, Croonian Lecture Fund.

Stevenson Bequest. Chancery Dividend. One-fourth annual interest on Bank Stock and other Securities (produced £609 15*s.* 11*d.* in 1890-91).

The Funds in Court standing to the credit of the cause were formerly:—

- £11,000 Bank Stock.
- £11,031 London and North Western Railway Consolidated 4 per Cent. Guaranteed Stock.
- £11,105 Great Northern Railway 4 per Cent. Perpetual Preference Stock.
- £11,031 North Eastern Railway Consolidated 4 per Cent. Guaranteed Stock.
- £8,894 Great Western Railway 5 per Cent. Consolidated Guaranteed Stock.
- £11,035 16*s.* 5*d.* Midland Railway 4 per Cent. Preference Stock.

Subject to certain charges, the Royal Society was entitled to one-fourth of the proceeds.

There has now been paid out of Court to the Society:—

- £2,128 9*s.* 7*d.* Bank Stock.
- £2,758 London and North Western Railway Consolidated 4 per Cent. Guaranteed Stock.
- £2,725 Great Northern Railway 4 per Cent. Perpetual Preference Stock.
- £2,760 North Eastern Railway Consolidated 4 per Cent. Guaranteed Stock.
- £370 3*s.* 7*d.* Midland Railway 4 per Cent. Perpetual Guaranteed Preference Stock.

£3,200 Mortgage Loan, $3\frac{1}{2}$ per Cent., to the Duke of Norfolk.

£19,482 8*s.* 8*d.*, $2\frac{3}{4}$ per Cent. Consolidated Stock — being £10,779 8*s.* 2*d.* on account of the following Funds:—

	£	*s.*	*d.*
Rumford Fund	2,330	0	0
Wintringham Fund	1,200	0	0
Gassiot Trust	400	0	0
Sir J. Copley Fund	1,666	13	4
Jodrell Fund	5,182	14	10

£5,185 0*s.* 3*d.* General Purposes.

and £3,518 0*s.* 3*d.* in Chancery, arising from sale of the Coleman Street Estate.—General Purposes.

£403 9*s.* 8*d.* New $2\frac{1}{2}$ per Cent. Stock.—Bakerian and Copley Medal Fund.

£3,000 India $3\frac{1}{2}$ per Cent. Stock.—General Purposes.

£800 Midland Railway 3 per Cent. Debenture Stock.—Keck Bequest.

£5,660 Madras Railway Guaranteed 5 per Cent. Stock { General Purposes, £5,000. / Davy Medal Fund, £660.

£10,000 Italian Irrigation Bonds.—The Gassiot Trust.

£8,528 Great Northern Railway 3 per Cent. Debenture Stock { Scientific Relief Fund, £6,666 13*s.* 4*d.* / The Trevelyan Bequest, £1,861 6*s.* 8*d.*

£5,030 Great Northern Railway Perpetual 4 per Cent. Guaranteed Stock.—Donation Fund.

£1,400 Metropolitan 3½ per Cent. Stock.—Fee Reduction Fund.

£7,000 London and North Western Railway 4 per Cent. Perpetual Debenture Stock.—Fee Reduction Fund.

£18,150 " " " " 4 per Cent. Consolidated Guaranteed Stock.— { £6,000 Scientific Relief Fund. / £12,150 General Purposes.

£5,000 " " " " Consolidated 4 per Cent. Preference Stock.—General Purposes.

£5,000 North Eastern Railway 4 per Cent. Preference Stock.—General Purposes.

£2,200 South Eastern Railway 4 per Cent. Debenture Stock.—Darwin Memorial Fund.

£4,340 South Eastern Railway 5 per Cent. Debenture Stock.—Scientific Relief Fund.

£3,333 London and South Western Railway 4 per Cent. Preference Stock.—General Purposes.

£4,798 Lancashire and Yorkshire Railway 4 per Cent. Guaranteed Stock.—Handley Fund.

£800 London, Brighton, and South Coast Railway Consolidated Guaranteed 5 per Cent. Stock.—Joule Memorial Fund.

£4,000 Southern Mahratta Railway 4 per Cent. Debenture Stock.—General Purposes.

£303 9*s.* 9*d.* on Deposit Account at Bank.—Brady Library Account.

£300 on Deposit Account on behalf of the Committee.—Joule Memorial Fund.

£1,000 Policy in the Atlas Assurance Office, becoming due October 7th, 1899.—Catalogue Account.

£1,000 Bond.—Dr. Gunning.—Interest to be applied to the promotion of Physics and Biology.

JOHN EVANS, *Treasurer.*

We, the Auditors of the Treasurer's Accounts on the part of the Council, have examined these Accounts and found them correct.

M. FOSTER.
W. T. BLANFORD.
F. D. GODMAN.

We, the Auditors of the Treasurer's Accounts on the part of the Society, have examined these Accounts and found them correct.

W. GRYLLS ADAMS.
ARTHUR W. RÜCKER.
W. C. WILLIAMSON.

Trust Funds. 1892.

Scientific Relief Fund.

£6,000 L. & N.W.R. 4 per Cent. Consolidated Guaranteed Stock.
£6,666 13*s.* 4*d.* Great Northern Railway 3 per Cent. Debenture Stock.
£4,340 South Eastern Railway 5 per Cent. Debenture Stock.

Dr	£	s.	d.	£	s.	d.	Cr.	£	s.	d.	£	s.	d.
To Balance { Income	970	1	8				By Grants				575	0	0
To Balance { Capital	480	9	9	1,450	11	5	,, Balance, Income	£1,047	14	7	1,533	4	4
,, Dividends				640	11	6	,, Capital	485	9	9			
,, Annual Subscription				5	0	0							
,, Interest on Deposit				12	1	5							
				£2,108	4	4					£2,108	4	4

Donation Fund.

£5,030 Great Northern Railway Perpetual 4 per Cent. Guaranteed Stock.
The Trevelyan Bequest. £1,861 6*s.* 8*d.* Great Northern Railway 3 per Cent. Debenture Stock.

	£	s.	d.		£	s.	d.
To Balance	876	14	11	By Grants	519	13	4
,, Dividends	250	12	2	,, Balance	746	13	1
,, Transfer from Jodrell Fund	188	19	4				
	£1,266	6	5		£1,266	6	5

Rumford Fund.

£2,330 $2\frac{3}{4}$ per Cent. Consolidated Stock.

	£	s.	d.		£	s.	d.
To Balance	145	18	6	By Balance	208	8	2
,, Dividends	62	9	8				
	£208	8	2		£208	8	2

Bakerian and Copley Medal Fund.

Sir Joseph Copley's Gift, £1,666 13*s.* 4*d.* $2\frac{3}{4}$ per Cent. Consolidated Stock.
£403 9*s.* 8*d.* New $2\frac{1}{2}$ per Cent. Stock.

	£	s.	d.		£	s.	d.
To Balance	111	0	6	By Gold Medal	4	12	0
Dividends, New $2\frac{1}{2}$ per Cent. Stock	9	16	8	,, Bakerian Lecture, Professor James Thomson	4	0	0
Dividend—Sir J. Copley's Fund	44	13	4	,, Balance	156	18	6
	£165	10	6		£165	10	6

The Keck Bequest.

£800 Midland Railway 3 per Cent. Debenture Stock.

	£	s.	d.		£	s.	d.
To Dividends	23	8	0	By Payment to Foreign Secretary	23	8	0

Wintringham Fund.

£1,200 $2\frac{3}{4}$ per Cent. Consolidated Stock.

	£	s.	d.		£	s.	d.
To Balance	32	4	0	By Payment to Foundling Hospital	32	4	0
,, Dividends	32	4	0	,, Balance	32	4	0
	£64	8	0		£64	8	0

Croonian Lecture Fund.

One-fifth of the clear rent of an Estate at Lambeth Hill, from the College of Physicians, about £52 per annum.

	£ s. d.		£ s. d.
To Rent	Nil	By Lecture (1892)	Nil
	Nil		Nil

Davy Medal Fund.

£660 Madras Railway Guaranteed 5 per Cent. Stock.

	£ s. d.		£ s. d.
To Balance	74 12 1	By Gold Medals	32 8 9
" Dividends	32 3 6	" Balance	74 6 10
	£106 15 7		£106 15 7

The Gassiot Trust.

£10,000 Italian Irrigation Bonds.

£400 $2\frac{3}{4}$ per Cent. Consolidated Stock.

	£ s. d.		£ s. d.
To Balance	62 16 6	By Payments to Kew Committee	487 10 0
" Dividends	498 4 8	" Purchase of £300 Italian Irrigation Bonds	334 18 0
" Bonds drawn	352 1 7	" Balance	90 14 9
	£913 2 9		£913 2 9

Handley Fund.

£4,798 Lancashire and Yorkshire Railway 4 per Cent. Guaranteed Stock.

	£ s. d.		£ s. d.
To Dividends	187 2 4	By Transfer to Catalogue Account	187 2 4

The Jodrell Fund.

£5,182 14*s.* 10*d.* 2¾ per Cent. Consolidated Stock.

	£	s.	d.		£	s.	d.
To Dividends	138	19	4	By Transfer to Donation Fund	138	19	4

Fee Reduction Fund.

£4,400 Metropolitan 3½ per Cent. Stock.
£7,000 London and North Western Railway 4 per Cent. Perpetual Debenture Stock.

	£	s.	d.		£	s.	d.
To Balance	204	15	1	By Transfer to Royal Society General Account	320	0	0
„ Dividends	423	3	0	„ Balance	307	18	1
	£627	18	1		£627	18	1

Darwin Memorial Fund.

£2,200 South Eastern Railway 4 per Cent. Debenture Stock.

	£	s.	d.		£	s.	d.
To Balance	293	18	0	By Balance	379	14	0
„ Dividends	85	16	0				
	£379	14	0		£379	14	0

Joule Memorial Fund.

£800 London, Brighton, and South Coast Railway Consolidated Guaranteed 5 per Cent. Stock.
£300 on Deposit on behalf of the Committee.

	£	s.	d.		£	s.	d.
To Balance	203	11	11	By Tablet and Abbey Fees	181	3	4
„ Interest on Deposit	3	12	2	„ Balance	65	0	9
„ Dividend	39	0	0				
	£246	4	1		£246	4	1

Brady Library Fund.

£303 9*s.* 9*d.* on Deposit Account at Bank.

	£	s.	d.		£	s.	d.
To Amount on Deposit at Bank	300	0	0	By Balance on Deposit	303	9	9
„ Interest thereon	3	9	9				
	£303	9	9		£303	9	9

Gunning Fund.

£1,000 4 per Cent. Bond of His Excellency Dr. Gunning.

	£	s.	d.		£	s.	d.
To Interest	40	0	0	By Balance	40	0	0

The following Table shows the progress and present state of the Society with respect to the number of Fellows :—

	Patron and Royal.	Foreign.	Compounders.	£4 yearly.	£3 yearly.	Total.
Nov. 30, 1891 ..	5	46	166	138	161	516
Since Elected ..			+ 3	+ 2	+12	+17
Since Deceased ..	−1	−4	−14	−13	− 2	−34
Since Compounded			+ 1		− 1	
Nov. 30, 1892 ..	4	42	156	127	170	499

Account of Grants from the Donation Fund in 1891–92.

	£	s.	d.
Dr. Woodward, to aid Dr. Hinde in illustrating his Memoir on Sponge Remains from the Lower Tertiary Strata of Oamaru, N.Z.	40	0	0
F. C. Penrose, for aid in Researches "on the Orientation of ancient Greek Temples"	100	0	0
Prof. W. N. Parker, for the completion of Researches on *Protopterus*	54	9	4
E. H. Griffiths, for Investigations into the Changes in the Specific Heat of Water, and the Determination of the Value of J	100	0	0
Dr. H. Gadow, for Investigations into the Anatomy of Elasmobranch Fishes	50	0	0
Prof. Schäfer, to aid Dr. Haycraft in Researches on the Intimate Nature of Secretion	30	0	0
Col. Godwin-Austen, to aid Mr. W. Doherty in the Collection of Land Mollusca in the Malay Archipelago ..	30	0	0
W. T. Thiselton Dyer, for obtaining Botanical Collections in an Expedition to Kilima-Njaro	50	0	0
Mountain Observatory Committee, for completing telescope	80	0	0
	£534	9	4
Repayments.............	14	16	0
	£519	13	4

December 8, 1892.

The LORD KELVIN, D.C.L., LL.D., President, followed by Sir JOHN EVANS, K.C.B., D.C.L., LL.D., Treasurer, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The President announced that he had appointed as Vice-Presidents—

The Treasurer.
Mr. W. T. Blanford.
Prof. G. C. Foster.
Prof. Lockyer.

The following Papers were read:—

I. "On the Photographic Spectra of some of the Brighter Stars." By J. NORMAN LOCKYER, F.R.S. Received November 3, 1892.

(Abstract.)

The present communication consists of a discussion of 443 photographs of the spectra of 171 stars, which have been obtained at Kensington and Westgate-on-Sea during the last two years.

The chief instrument employed in this work has been a 6-inch refracting telescope in conjunction with—at different times—objective prisms of $7\frac{1}{2}°$ and 45° respectively.

By this method the time of exposure is short, and good definition, with large dispersion, is easily secured. The spectra thus obtained will bear enlargement up to thirty times without much sacrifice of definition.

The 30-inch reflector and slit-spectroscope at Westgate-on-Sea have also been used in the inquiry.

My object has not been so much to obtain photographs of the spectra of a large number of stars as to study in detail the spectra of comparatively few.

In the classifications of stars adopted by others from a consideration of the visual observations, only the broader differences in the spectra have been taken into account. Professor Pickering has more recently employed a provisional classification in connexion with the Henry Draper Memorial photographs of stellar spectra, but this chiefly relates to photographs taken with small dispersion. With

larger dispersion it becomes necessary to deal with the presence or absence of individual lines.

In the first instance, the various stars of which the spectra have been photographed at Kensington have been arranged in tables, without reference to any of the existing classifications, and taking into account the finer details. The basis on which the main tabular divisions of the spectra are founded is the amount of continuous absorption at the blue end. This distinction was not possible in the case of the eye observations.

The stars included in the first table are characterised by the absence of any remarkable continuous absorption at the blue end, and by the presence in their spectra of broad lines of hydrogen. These have been further classified in four sub-divisions, depending on the presence or absence of other lines.

In the stars of the second table there is a considerable amount of continuous absorption in the ultra-violet, and the spectra beyond K are very difficult to photograph as compared with the stars of the first table. In these stars the thickness of the hydrogen lines is about the same as in the solar spectrum. These also are arranged in two sub-divisions.

In all the stars included in the third table there is a very considerable amount of continuous absorption in the violet, extending to about G, and it is a matter of great difficulty to photograph these spectra, as most of the stars of this class are below the 3rd magnitude. The hydrogen lines are very thin. One sub-division includes the spectra which show flutings shading away towards the less refrangible end of the spectrum. The other comprises stars without flutings in their spectra. The brightest star in this table, α Orionis, is discussed in detail, the result tending to show that the temperature of the absorbing iron vapours is not much greater than that of the oxy-hydrogen flame.

The relations of the various sub-divisions to which reference has been made are then traced.

One important fact comes out very clearly, namely, that whether we take the varying thicknesses of the hydrogen lines or of the lines of other substances as the basis for the arrangement of the spectra, it is not possible to place all the stars in one line of temperature. Thus, there are stars in which the hydrogen lines are of the same average thickness, while the remaining lines are almost entirely different. These spectra cannot, therefore, be placed in juxta-position, and it is necessary to arrange the stars in two series.

The next part of the paper consists of a discussion of the photographic results in relation to the meteoritic hypothesis. In the Bakerian Lecture for 1888, I brought together the various observations of the spectra of stars, comets, and nebulæ, and the discussion

suggested the hypothesis that all celestial bodies are, or have been, swarms of meteorites, the difference between them being due to different stages of condensation. The new classification rendered necessary by this hypothesis differed from previous ones, inasmuch as the line of evolution followed, instead of locating the highest temperature at its commencement, as demanded by Laplace's hypothesis, placed it much later. Hence bodies of increasing temperature were demanded as well as bodies of decreasing temperature.

The question how far this condition is satisfied by the new facts revealed by the photographs is next discussed.

This involves the consideration of some points in connexion with the hypothesis to which brief reference alone has been made in previous communications. The phenomena to be expected on the hypothesis, and the actual facts, are given side by side below.

Nebulæ.

The bright lines seen in nebulæ should have three origins:—	
(1.) The lines of those substances which occupy the interspaces between the meteorites. Chief among these, from laboratory experiments, we should expect hydrogen and gaseous compounds of carbon.	(1.) Lines at wave-lengths approximately very closely to the lines of hydrogen, and to some of the carbon flutings, appear in the spectra of nebulæ.
(2.) The most numerous collisions between the meteorites will be partial ones—mere grazes—sufficient only to produce comparatively slight rises of temperature.	(2.) There is a fluting most probably due to magnesium at λ 500, and the longest flame lines of iron, calcium, and magnesium are seen.
(3.) There will, no doubt, be a small number of end-on collisions, producing very high temperatures, and there should be evidence of some high-temperature lines.	(3.) The chromospheric line D_3 and another line at λ 4471 (which is always associated with D_3 in the chromosphere) have been recorded in the spectrum of the Orion Nebula.

Bright-Line Stars.

The lines seen in the spectra of bright-line stars should, in the main, resemble those which appear in nebulæ. They will differ, however, for two reasons given in the paper.	Professor Pickering has shown that the Draper Memorial photographs prove that bright-line stars are intimately connected with the planetary nebulæ, the lines in the spectra being almost identical.

Stars of Increasing Temperature.

Stage 1.—Immediately following the stage of condensation giving bright-line stars, the bright lines from the interspaces will be masked by corresponding dark ones, due to absorption of the same vapours surrounding the incandescent meteorites, and these lines will therefore vanish from the spectrum.

The spectra of stars given in the third table answer these requirements. They show no bright lines under normal conditions.

Owing to the interspaces being restricted, absorption phenomena will be in excess, and low-temperature metallic fluting absorption will first appear. The radiation spectrum of the interspaces will now consist chiefly of carbon.

The dark flutings in the visual spectrum agree very closely in position with the flutings seen in the flame spectra of manganese, lead, and iron. The evidence afforded by the photographs proves the actual presence of carbon radiation.

Under these conditions the amount of continuous absorption at the blue end will be at a maximum.

The photographs show a considerable amount of continuous absorption in the ultra-violet and violet.

Stage 2.—With further condensation, the radiation spectrum of the interspaces will gradually disappear, and dark lines replace the fluting absorption owing to increase of temperature, though this line absorption need not necessarily resemble that in the solar spectrum.

The spectra consist of numerous dark metallic lines, but they do not exactly resemble the solar spectrum. α Tauri and γ Cygni are types of stars at this stage.

Stage 3.—(1.) The line absorption and the continuous spectrum at the blue end will diminish as the condensations are reduced in number, as only those vapours high up in the atmospheres surrounding the condensations will be competent to show absorption phenomena in consequence of the bright continuous spectrum of the still disturbed lower levels of those atmospheres.

(1.) These conditions are satisfied by such stars as α Cygni, Rigel, Bellatrix, δ Orionis, and α Virginis. In these there is no continuous absorption at the blue end, the spectra consisting of simple line absorption.

(2.) Lines of iron and other substances will disappear at this stage, because the bright lines from the interspaces will counteract the lines in the same positions due to absorption of surrounding vapours.

(2.) In the spectrum of α Cygni, which represents the earliest example of this stage, there are a few of the longest lines of iron, but in other stars of this class the iron lines disappear.

(3.) The chances of violent collisions being now enormously increased, we should expect the absorption of very high-temperature vapours. The solar chromospheric lines may be taken as examples of lines produced at such temperatures.

(3.) The new lines which now appear include the chromospheric line at λ 4471, and possibly a few others.

The Hottest Stars.

The order of the absorbing layers should follow the original order of the extension of the vapours round the meteorites in the first condition of the swarm, and the lines seen bright in nebulæ, whatever their origins may be, should therefore appear almost alone as dark lines.

In stars like α Andromedæ we have absorption lines agreeing in position with some of the bright lines which appear in nebulæ.

Stars of Decreasing Temperature.

Stage 1.—Owing to the diminishing depth of the absorbing atmosphere, the hydrogen lines will, on the whole, get thinner, and new lines will appear. These new lines will not necessarily be identical with those observed in the spectra of stars of increasing temperature. In the latter there will be the perpetual explosions of the meteorites affecting the atmospheres, whereas in a cooling mass of vapour we get the absorption of the highest layers of vapours. The first lines

Taking Sirius as a type of stars in the first stage of decreasing temperature, it is found

to appear, however, will be the longest low-temperature lines of the various chemical elements.

that its spectrum shows many of the longest lines of iron.

Stage 2.—The hydrogen lines will continue to thin out, and the spectra will show many more of the high-temperature lines of different elements. These will differ from the lines seen in stars of increasing temperature owing to the different percentage composition of the absorbing layers, so far as the known lines are concerned.

The conditions at this stage of cooling are satisfied by such stars as β Arietis and α Persei. In the spectrum of these stars nearly all the solar lines are found, in addition to fairly broad lines of hydrogen.

Stage 3.—With the further thinning out of the hydrogen lines and reduction of temperature of the atmosphere, the absorption flutings of the compounds of carbon should come in.

There is undoubted evidence of the presence of carbon absorption in the solar spectrum and the spectrum of Arcturus, the only star which has yet been investigated with special reference to this point.

The photographs, then, give us the same results as the one formerly obtained from the eye observations.

Comparison is then made between the groups in the classification first suggested by the eye observations, and the various sub-divisions in which the photographs have been arranged.

II. "On the Velocity of Crookes' Cathode Stream." By LORD KELVIN, P.R.S. Received December 3, 1892.

In connection with his splendid discovery of the cathode stream (stream from the cathode in exhausted glass vessels subjected to electric force), Crookes found that when the whole of the stream, or a large part of the whole, is so directed as to fall on 2 or 3 sq. cm. of the containing vessel, this part of the glass becomes rapidly heated up to many degrees, as much as 200° or 300° sometimes, above the temperature of the surroundings.

Let v be the velocity, in centimetres per second, of the cathode stream, and ρ the quantity of matter of all the molecules in 1 c.c. of it. Supposing what Crookes' experiments seem to prove to be not far from the truth, that their impact on the glass is like that of inelastic bodies, and that it spends all their translational energy in heating the glass. The energy thus spent, per square centimetre

of surface struck, per second of time, is $\frac{1}{2}\rho v^3$; of which the equivalent in gramme-water-centigrade thermal units is approximately $\frac{1}{2}\rho v^3/42,000,000$. The initial rate at which this will warm the glass, in degrees centigrade per second, is

$$\frac{\frac{1}{2}\rho v^3}{10^6 \times 42 \, . \, \sigma a} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (1),$$

where σ denotes the specific heat of the glass, and a the thickness of it at the place where the stream strikes it.

The limiting temperature to which this will raise the glass is

$$\frac{1}{\mathrm{E}} \times \frac{\frac{1}{2}\rho v^3}{42,000,000} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (2)$$

where E denotes the sum of the emissivities of the two surfaces of the glass in the actual circumstances.

It is probable that ρ differs considerably from the average density of the residual air in the enclosure. Let us take, however, for a conceivably possible example, $\rho = 10^{-8}$, which is what the mean density of the enclosed air would be if the vessel were exhausted to 8×10^{-8} of the ordinary atmospheric density.

To complete the example, take

$$v = 100,000 \text{ cm. per sec.}$$

(being about twice the average velocity of the molecules of ordinary air at ordinary temperature); and take

$$\sigma a = \tfrac{1}{8} \text{ cm.},$$

as it might be for an ordinary glass vacuum bulb; and take

$$\mathrm{E} = \tfrac{1}{3000},$$

which may not be very far from the truth.

With these assumptions, we find, by (1) and (2) approximately, 1° per second for the initial rise, and 375° for the final temperature, which are not very unlike the results found in some of Crookes' experiments.

The pressure of the cathode stream of the velocity and density which we have assumed by way of example is ρv^2, or 100 dynes per square centimetre, or about 100 milligrams heaviness per square centimetre, which is ample for Crookes' wonderful mechanical results.

The very moderate velocity of 1 kilom. per second which we have assumed is much too small to show itself by the optical colour test. The fact that this test has been applied, and that no indication of velocity of the luminous molecules has been found, has, therefore, no validity as an objection against Crookes' doctrine of the cathode stream.

III. "Experiments in Examination of the Peripheral Distribution of the Fibres of the Posterior Roots of some Spinal Nerves." By C. S. SHERRINGTON, M.A., M.D., Lecturer on Physiology at St. Thomas's Hospital, London. Communicated by Professor M. FOSTER, Sec. R.S. Received December 2, 1892.

(From the Physiological Laboratory of St. Thomas's Hospital, London.)

(Abstract.)

After reference to the experimental researches of Eckhard, Peyer, Krause, Koschewnikoff, Meyer, and Türck, and to the anatomical of Herringham and Paterson, mention is made of clinical observations by Thorburn, Starr, Mackenzie, Head, and others. The methods employed by the author in experiments on the Frog, Cat, and Monkey are then detailed. In the two latter animals the effect of consecutive sections in ascending or descending series upon the reflex movement elicitable by electrical excitation of the central end of a peripheral nerve was observed, and followed as a guide to the central connexions of the nerve. In the first- and last-mentioned animals mechanical excitation of the cutaneous surface, after previous section of a number of posterior roots above and below the root to be examined, was employed to detect the peripheral distribution of the nerve-root in the skin. The various experiments performed are individually described, and the results of each series are collated. The cutaneous fields of the thoracic and post-thoracic afferent spinal roots are shown in photographs, and in sketches made at the time in the laboratory. The photographs illustrate also the chief outlines of the segmental skin-fields of the cervical sensory roots; but description and discussion of the roots, above the 1st thoracic, are reserved for a future communication.

The author finally proceeds to draw conclusions of the following nature:—

The field of skin belonging to each sensory spinal root may be called the segmental skin-field. In each segmental field reflex reaction is less easily elicitable near the edge of the field than from the field elsewhere. The segmental fields do not present the same configuration as do the fields of the peripheral nerves.

Although in a plexus each posterior spinal root gives separate contributions to many several nerve-trunks, the cutaneous distribution of the root is composed not of patches which are disjoined, but of patches which are so joined that the distribution of the entire root forms one continuous field. Similarity of the root composition of neigh-

bouring nerve-twigs, that are near their destination, is a necessity of this arrangement. Thus, the dorsal collateral digital nerve, on the tibial side of a digit, will resemble, in composition, the plantar collateral digital on the tibial side of the same digit, although they are derived from separate parent trunks. This is comparable to the similarity of root composition exhibited by the several motor twigs entering one and the same muscle. Thus, the tibialis anticus frequently receives fibres from three motor spinal roots, and receives those fibres by at least three several nerve-branches entering it; the root composition of each of the nerve-branches is approximately the same. The dorsal digital supplying the cleft between the 1st and 2nd digits is interpolated in the series of digitals from the musculo-cutaneous nerve, although it comes itself from the anterior tibial. Yet it, by its root composition, falls into perfect series with the other digital nerves.

Each segmental skin-field spreads, to a certain extent, across neighbouring segmental skin-fields. It has a *fore-lap* extending into segmental fields immediately anterior to it; and an *after-lap* extending into fields immediately posterior; it has also cross-laps extending into the corresponding fields on the other lateral half of the body, both at the mid-dorsal line (the *dorsal cross-lap*) and the mid-ventral line (the *ventral cross-lap*). The *fore-lap* and the *after-lap* are, throughout the body, very great, and each region of skin appears to be supplied by at least two sensory spinal roots. The over-lap of the skin-fields of the individual filaments of a posterior root is great.

The shape of a segmental skin-field is, where simplest, *e.g.*, in the trunk and neck, band-like, wrapping transversely round one lateral half of the body; it has fairly parallel edges, but is somewhat broader near its ventral than at its dorsal end. In the limb the segmental skin-fields are *distorted* from the simple band-like type. The distortion of each segmental field in the hind-limb, and of some in the fore-limb, is, in the full paper, analysed, and for each the true anterior border, the true posterior border, and the true dorsal and ventra borders are found. This analysis is only possible after it has been recognised that in the limb the cutaneous segments are not only distorted but are seemingly *dislocated* from their attachments to the mid-dorsal and mid-ventral lines of the trunk.

The mid-dorsal line of the body may be said to, in the region of the limb, extend outwards as a side branch, a secondary axis, almost at right angles to itself. The same is done in the same region by the mid-ventral line. Upon these dorsal and ventral side lines as upon secondary dorsal and ventral axes, the cutaneous segments of the limb are ranged, as though upon folded portions of the axial lines of the trunk itself.

The axial lines for the hind-limb slant outward from the trunk

axis in a somewhat backward as well as lateral direction; those for the fore-limb conversely slope somewhat forward as well as outward. The *dorsal axial line* in the hind-limb runs from the mid-dorsum over the sacrum, past the back of the hip-joint, and along the outer face of the thigh nearly to the knee. Of the dorsal axial line of the fore limb only the most proximal part is described by the present experiments; that part runs from the mid-dorsum outwards and forwards over the infra-spinous fossa of the scapula. The *ventral axial line* of the hind-limb runs from the front of the body of the pubes to the inner border of the thigh, and descends along the junction of the extensor and adductor groups of muscles nearly to the knee. The *ventral axial line* of the fore-limb is in the present experiments only followed for its proximal part; that part lies on the chest close below the clavicle.

The position of these secondary axes having once been found (by methods described in the paper) in the limb, it is not difficult to examine the degree of apparent dislocation of each segmental field and the nature of its distortion. In the segmentally anterior aspect of the limb each segmental field has been curved so as to present a very convex posterior edge, and the after-lap of the field is very large. In the segmentally posterior aspect of the limb each segmental field has been curved so as to present a very convex anterior edge, and the fore-lap of the field is very large. The dorsal and ventral borders of the fields are, in the limb, not much increased in length. Owing to their serial arrangement along the secondary *mid-dorsum* and *mid-venter* of the limb there is a secondary cross-lap of the fields there of such a kind that a segmental field may there cross-lap with a segmental field far distant from it in the segmental series; thus the 9th post-thoracic field may cross-lap with the 4th post-thoracic field.

This dislocation of some of the segmental fields in the limb from the *mid-dorsum* and *mid-venter* is apparent rather than real, and is not a fundamental character of the limb segmentation, for it does not occur in primitive types, *e.g.*, is absent from the pelvic limb of the Frog.

Using the cutaneous field as a guide to the morphological position of various points in the body, it is seen that the edges of the foot and hand are, in the segmental fields of the limb, about midway between the mid-dorsal and mid-ventral lines, and therefore must correspond about with the lateral line of the trunk. The digits are therefore buds from the region of the lateral line.

The vulva and the anus are not at the posterior pole of the body, but, like the umbilicus, are placed in the mid-ventral line.

From the motor roots it is not easy to get evidence that the 1st digit of the foot or hand is segmentally anterior to the 5th digit; the root supply of the intrinsic musculature of each is so similar. But

from the sensory roots it is easy to show that the skin of the 1st digit is segmentally anterior to that of the 2nd digit, that of 2nd to that of 3rd, and so on. The skin of the dorsum of the foot is shown to be segmentally anterior to the sole.

The nipple lies in the middle of the 4th thoracic field, but is also included in the fields of the 3rd and 5th thoracic. The umbilicus is in the 11th thoracic root field.

The number of segments entering into the composition of the skin of the limb is seen to be greater than the number of segments contributing to its musculature. To the skin of the anterior aspect of the fore-limb, six segments contribute (3rd, 4th, 5th, 6th, 7th, 8th cervical); to that of the hind-limb six segments also (1st, 2nd, 3rd, 4th, 5th, 6th post-thoracic). To the posterior part of the fore-limb four segments contribute (1st, 2nd, 3rd, 4th thoracic); to that of the hind-limb four segments also (6th, 7th, 8th, 9th post-thoracic).

In each limb the anterior aspect is segmentally more extensive than the posterior. I have shown that this last fact is exemplified even more strikingly in the musculature of each limb.

The quadrifid or quinquifid digital partition at the free end of the limb gives no indication of the number of segmental skin fields in it.

Joints such as knee and ankle, which might perhaps seem natural boundaries marking fundamentally distinct portions of the limb, are not regarded as such in the segmentation of the cord, as evidenced by posterior roots.

The absolute segmental level of a point of surface is subject to individual variation, as was shown to be the case with muscular points in the substance of the body wall and viscera. This individual variation affecting the skin corresponds with variation in the constitution of the efferent roots; the limb plexus may be *postfixed* or *prefixed* by its sensory spinal roots, just as it may be by its motor spinal roots. A mixed nerve may be postfixed by its motor roots and by its sensory in the same individual, or may be prefixed by both. But there is some evidence (Frog) that a plexus may be prefixed by its motor roots when it is not so by its sensory roots, and *vice versâ*.

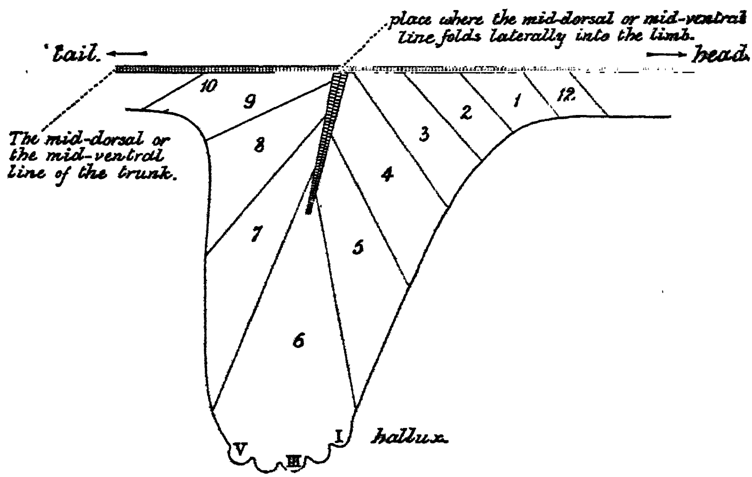
The distribution of the fibres of the sensory spinal root in the limb, as elsewhere, indicates a segmental significance in their constitution rather than a functional based upon coordination. Without denying the existence of functional factors in the progressive development of the limb, it must be admitted that there is little evidence that the collection of fibres in each sensory root has resulted from an assortment of the fibres with a view towards assisting in functional coördination.

Peyer's statement of the correspondence of locality of the muscular and cutaneous distributions of a spinal nerve does not apply in the Monkey. The 9th post-thoracic nerve innervates the intrinsic

muscles of the foot, but its skin-field is on the buttock. In the lower half of the trunk and in the pelvic limb the skin at any point will be supplied by sensory spinal roots segmentally anterior to (higher than) the spinal motor root supplying the subjacent muscle. A partial exception to this will be in the skin of the back of the thigh, where, for reasons explained, the supply of both may be from the same segmental level.

The cutaneous fields of the posterior spinal roots do not correspond with the fields of cutaneous distribution of the motor roots, as judged by the pilomotor fibres of those roots. The pilomotor fields and the cutaneous sensory fields do not correspond.

As to cutaneous vasomotor fields and the cutaneous sensory fields, there does in *Macacus rhesus* seem to be a curious correspondence of the area of "sexual" skin at the root of the tail, on the buttock, and along the back of the thigh with the combined sensory skin field of 10th, 9th, and 8th post-thoracic roots.



Cutaneous Segments of the Pelvic Limb of Monkey, dorsal or ventral aspect. (The overlapping of the segments is not shown.)

IV. "Preliminary Account of the Nephridia and Body Cavity of the Larva of *Palæmonetes varians.*" By EDGAR J. ALLEN, B.Sc., University College, London. Communicated by Professor W. F. R. WELDON, F.R.S. Received November 17, 1892.

The researches of which the present communication contains a brief summary, were carried on during the summer of the present year, at the Laboratory of the Marine Biological Association in Plymouth, where I occupied a table by appointment of the British Association Committee. The observations were made chiefly on larvæ of *Palæmonetes varians*, but other species have also been included, and will be mentioned in particular instances.

I. *The Nephridia.*

During the greater part of the larval life two pairs of nephridia are present: the green glands, which open at the bases of the second antennæ, and the shell glands, which open at the bases of the second maxillæ.

The Green Gland.—In a larva of *Palæmonetes* which is a few days old, the green gland has a form similar to that described by Weldon* and Marchal† for the adult of *Virbius*, *Pandalus*, and *Crangon*, excepting that the remarkable enlargements of the bladder, which the former author has termed "nephroperitoneal sacs," are not as yet developed. The gland consists of an end sac, which communicates by means of a U-shaped tube with a very short ureter opening at the base of the antenna. The distal portion of the tube is slightly enlarged, and may be called the bladder. At the time of hatching of the larva, the whole gland consists of a solid mass of cells, in which no cavity has appeared, although the portions which afterwards form the end sac and the tube can be distinguished, and the ureter and external opening are already present. Shortly after the larva has become free the cells separate, and give rise to the lumen of the gland.

The later development of the green gland consists chiefly in the enlargement of the bladder, which grows at first inwards, towards the middle ventral line of the body, then upwards, within the œsophageal nerve ring, and anterior to the œsophagus, to the middle

* Weldon, W. F. R., "The Renal Organs of certain Decapod Crustacea." 'Quart. Journ. Micr. Sci.,' vol. 32, 1891.

† Marchal, P., "Recherches anatomiques et physiologiques sur l'appareil excréteur des Crustacés Décapodes." 'Arch. Zool. Expér.,' vol. 10, 1892.

dorsal line, where it meets its fellow of the opposite side. The two bladders grow backwards over the stomach, subsequently fusing in the middle line to form the unpaired nephroperitoneal sac. This mode of development confirms the view as to the nature of the latter sac already arrived at by Weldon and Marchal from a comparative study of the renal organs of Decapods.

The Shell Gland.—In a figure of a *Callianassa* larva in the *Mysis* stage, Claus* inserts and names the shell gland, opening at the base of the second maxilla. This is, I believe, the only recorded instance of the gland having been recognised in a Decapod, unless, indeed, the "segmental organ" described by Lebedinski† as opening at the base of the first maxillipede of the larva of *Eriphya spinifrons*, and communicating with the body cavity, be the same organ.

In late embryos, and at the time of hatching of the larva, the shell glands are the functional kidneys of *Palæmonetes* and *Palæmon*, the green gland being still without a lumen. The shell gland of *Palæmonetes* consists of a comparatively short renal tube, with a considerable lumen, which communicates internally with an end sac, and opens externally at the base of the second maxilla. The general form of the tube may be expressed by saying that it is Y-shaped, the two arms of the Y being in a horizontal plane, with the end sac attached to the internal one, whilst the leg of the Y is curved in a vertical plane, the concavity looking downwards and backwards. The histological structure of both end sac and renal tube is similar to that described by Grobben‡ for the green gland of *Mysis*. The entrance from the end sac to the tube is guarded, however, by a valve, formed of elongated cells of the end sac, which does not appear to have been found in other forms.

I have detected no trace of the shell gland in young adults.

II. *The Body Cavity.*

The Anterior Region of the Thorax.—A transverse section through the region of the second maxillæ of a *Palæmonetes* larva, which is a few days old, shows that the cavity enclosed by the external chitin and ectoderm may be divided into four regions: a *dorsal sac*, surrounded by a definite layer of epithelium, and within which the cephalic aorta lies, but which does not itself contain blood; a *central cavity*, in which the liver, intestine, and nerve cord, are found; two

* Claus, C., "Neue Beiträge zur Morphologie der Crustaceen." 'Arb. Zool. Inst. Wien,' vol. 6, 1886.

† Lebedinski, J., "Einige Untersuchungen über die Entwicklungsgeschichte der Seekrabben." 'Biol. Centralbl.,' vol. 10, 1890.

‡ Grobben, C., "Die Antennendrüse der Crustaceen." 'Arb. Zool. Inst. Wien,' vol. 3, 1881.

lateral cavities, separated from the central cavities by masses of muscle and bands of connective tissue, and which, in the region under consideration, contain the proximal ends of the shell glands; and, fourthly, the *cavities of the limbs*, which contain the distal ends of the same organs. The cavities of the limbs communicate with the lateral cavities, and the latter frequently communicate with the central cavity by the disappearance of the connective tissue bands. The central cavity, the lateral cavities, and the cavities of the limbs, all contain blood.

The Dorsal Sac.—I have found the dorsal sac in *Palæmon serratus*, *Palæmonetes varians*, and *Crangon vulgaris*, and in the adult it attains a considerable size. If a dissection be made of an adult *Palæmon*, the sac is readily seen. Anteriorly it appears as an elongated, cylindrical tube, lying upon the nephroperitoneal sac, and containing within it the cephalic aorta. Posteriorly it is very much enlarged, covering the front part of the ovaries, and running downwards on either side into the cavity which surrounds the intestine and liver. A similar condition of things is found in the adult *Palæmonetes*.

The dorsal sac does not contain blood. I have been led to this conclusion for the following reasons:—(1.) In a large number of series of sections, both of larvæ and adults, I have never seen a blood corpuscle within the sac. (2.) The sac is completely closed, and has no communication with the blood sinuses of the body. In preserved specimens it contains a clot, which can generally be distinguished from the surrounding blood clot. (3.) I have observed carefully, and for a long time, living larvæ, and the space occupied by the sac has always been perfectly free from blood corpuscles.

At its anterior end the dorsal sac is surrounded by a mass of tissue, from the external surface of which blood corpuscles appear to be budding off. Before commencing this research, Professor Weldon, to whom the existence of this tissue was known, and who has indicated it in his figures, suggested to me this view of its nature, and it is fully supported by my preparations.

Development of the Dorsal Sac.—In embryos of *Palæmonetes* in which the cephalic aorta is already formed, the cells surrounding the latter are arranged in two layers, an internal and an external. Before the time of hatching arrives, the cells of the external layer enlarge considerably, and give rise to the appearance of a solid mass of cells upon either side of the aorta. The dorsal sac is formed by the hollowing out of these masses of cells. Two lateral cavities are thus formed, which are separated by the aorta. The protoplasm of the cells lining these cavities, which is at first gathered into masses around the nuclei, then spreads out into a thin sheet, drawing away from the lower portion of the aorta, and causing the two lateral cavities to unite ventrally, and so form a single sac. In the region of the first

and second maxillæ, all the stages of the process just described may be seen. In the region anterior to this I have never actually observed the stage with two lateral cavities, the two having always united ventrally, but I do not doubt that the process is here the same as in the posterior region.

In one series of sections of a larva, preserved very soon after it had left the egg, the cavity was formed upon either side of and below the aorta, as far back as the anterior end of the segment of the first maxillæ, where for one section it was almost completely closed. Behind this the condition with two lateral cavities was found, and persisted through the region of the first maxillæ, whilst in the region of the second maxillæ no cavity had yet opened, and solid masses of cells still lay upon either side of the aorta.

The further development of the dorsal sac consists mainly of an increase in its size. At its posterior end it grows backwards in a pair of lobes, which extend as far as the front end of the pericardium.

The Posterior Region of the Thorax.—The central and lateral cavities are here similar to those of the anterior region, whilst dorsal to them the pericardial chamber lies. This chamber is separated from the central body cavity, as is already well known, by the pericardial septum, and it contains the heart. The genital organs are situated at the front end of the pericardium, immediately below the pericardial septum. In the just-hatched larva these consist of two masses of cells with large nuclei, each mass being enclosed in a sheath of mesoderm. I have not detected any trace of the genital ducts at this stage.

The Abdomen.—With regard to the abdomen, my sections confirm the accounts given by Milne Edwards* and Claus.† There are two main sinuses, which run along its length: a dorsal sinus, in which the intestine lies, and a ventral one, which contains the nerve cord. These two sinuses are generally separated by masses of muscle, but they communicate at intervals by means of lateral sinuses.

Theoretical Considerations.

The state of the body cavity in the anterior region of the thorax of *Palæmonetes* may be compared with that of *Peripatus*, as described by Sedgwick,‡ at the time when the dorsal portions of the mesoblastic somites have attained their maximum development. Bearing in mind that the dorsal sac of *Palæmonetes* has been formed by the

* Milne Edwards, 'Histoire naturelle des Crustacés,' Paris, 1834.

† Claus, C., "Zur Kenntniss der Kreislaufsorgane der Schizopoden und Decapoden." 'Arb. Zool. Inst. Wien,' vol. 5, 1884.

‡ Sedgwick, A., "The Development of the Cape Species of *Peripatus*." Parts I—IV. 'Quart. Journ. Micr. Sci.,' vols. 25—28, 1885–88.

union of two lateral cavities, which lay on either side of the aorta, the differences between the two forms are very slight. The dorsal sac represents the two dorsal portions of the mesoblastic somites, whilst the central cavity, the lateral cavities, and the nephridia agree, with the one exception that the two lateral portions of the nerve cord of *Peripatus* have united in the middle line in *Palæmonetes*, and in the process have passed out of the lateral cavities. The agreement is so close that it appears to me to be fully justifiable to homologise the various parts. If this be so, it follows that the dorsal sac of *Palæmonetes* is homologous with the dorsal portions of the mesoblastic somites of *Peripatus*, and that its cavity is a true cœlom. The central and lateral cavities, together with the cavities of the legs, will represent the pseudocœle, and being filled with blood may be termed, with Lankester, *hæmocœle*.

Passing now to the posterior part of the thorax, the region of the heart, we find that the different cavities correspond with those which persist in the adult *Peripatus*. Heart, pericardium, and pericardial septum of *Palæmonetes* present exactly the same relations as in *Peripatus*, and are clearly homologous structures in the two animals. The central and lateral cavities only differ on account of the relative position of the nervous system, and this difference has already been accounted for. It must be noted, however, that in this region no nephridia are found in the cavities of the limbs. Beneath the anterior end of the pericardial septum are found, as has been already stated, the genital organs, and here also the comparison with *Peripatus* may be instituted. We find a similar agreement to that which existed in the other regions compared, and we may, with a considerable degree of probability, again homologise corresponding parts. The genital organs of *Palæmonetes* must then be regarded as the representatives, in this region, of the cœlom.

If the homologies here suggested are valid, the body cavity relations of the Crustaceans under consideration may be stated briefly thus: both enterocœle (true cœlom) and pseudocœle are present, the enterocœle consisting of the dorsal sac, the green gland, and shell gland, or the end sacs of these organs, together with the genital organs and their ducts, whilst the pericardial septum may be regarded as equivalent to portions of the walls of another part of the same structure.

The pseudocœle consists of the heart and arteries, the pericardial cavity, the central cavity of the thorax, with the lateral cavities and the cavities of the limbs, and the various sinuses of the abdomen. The pseudocœle is filled with blood, and hence can be termed a hæmocœle.

I hope, shortly, to publish a more detailed account, with figures, of the relations described in this communication.

Presents, December 8, 1892.

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December 15, 1892.

Sir JOHN EVANS, K.C.B., D.C.L., LL.D., Treasurer, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The Right Hon. John Morley, a member of Her Majesty's Most Honourable Privy Council, whose certificate had been suspended as required by the Statutes, was balloted for and elected a Fellow of the Society.

The following Papers were read:—

I. "On an Apparatus for facilitating the Reduction of Tidal Observations." By G. H. DARWIN, F.R.S., Plumian Professor and Fellow of Trinity College, Cambridge. Received November 12, 1892.

§ 1. *Introduction.*

The tidal oscillation of the ocean may be represented as the sum of a number of simple harmonic waves which go through their periods approximately once, twice, thrice, four times in a mean solar day. But these simple harmonic waves may be regarded as being rigorously diurnal, semi-diurnal, ter-diurnal, and so forth, if the length of the day referred to be adapted to suit the particular wave under consideration. The idea of a series of special scales of time is thus introduced, each time-scale being appropriate to a special tide. For example, the mean interval between successive culminations of the moon is 24^h 50^m, and this interval may be described as the mean lunar day. Now there is a series of tides, bearing the initials M_1, M_2, M_3, M_4, &c., which go through their periods rigorously once, twice, thrice, four times, &c., in a mean lunar day. The solar tides, S, proceed according to mean solar time; but, besides mean lunar and mean solar times, there are special time scales appropriate to the larger (N) and smaller (L) lunar elliptic tides, to the evectional (ν), to the diurnal (K_1) and semi-diurnal (K_2) luni-solar tides, to the lunar diurnal (O), &c.

The process of reduction consists of the determination of the mean height of the water at each of 24 special hours, and subsequent harmonic analysis. The means are taken over such periods of time that the influence of all the tides governed by other special times is eliminated.

The process by which the special hourly heights have hitherto been obtained is the entry of the heights observed at the mean solar hours in a schedule so arranged that each entry falls into a column appropriate to the nearest special hour. Schedules of this kind were prepared by Mr. Roberts for the Indian Government.* The successive rearrangements for each sort of special time were made by recopying the whole of the observations time after time into a series of appropriate schedules. The mere clerical labour of this work is enormous, and great care is required to avoid mistakes.

All this copying might be avoided if the observed heights were written on movable pieces. But a year of observation gives 8,760 hourly heights, and the orderly sorting and re-sorting of nearly 9,000 pieces of paper or tablets might prove more laborious and more treacherous than recopying the figures.

It occurred to me, however, that the marshalling of movable pieces might be reduced to manageable limits if all the 24 observations pertaining to a single mean solar day were moved together, for the movable pieces would be at once reduced to 365, and each piece might be of a size convenient to handle.

The realisation of this plan affords the subject of this paper, and it will appear that not only is all desirable accuracy attainable, but that the other requisite of such a scheme is satisfied, namely, that the whole computing apparatus shall serve any number of times and for any number of places.

The first idea which naturally occurred was to have narrow sliding tablets which should be thrown into their places by a number of templates. It is unnecessary to recount all my trials and failures, but it will suffice to say that the slides and templates require the precision of a mathematical instrument if they are to work satisfactorily, and that the manufacture would be so expensive as to make the price of the instrument prohibitive.

The idea of making the tablets or strips to slide into their places was then abandoned, and the strips are now made with short pins on their under sides, so that they can be stuck on to a drawing board in any desired position. The templates, which were also troublesome to make, are replaced by large sheets of paper with numbered marks on

* An edition of these computation forms was reprinted by aid of a grant from the Royal Society, and is sold by the Cambridge Scientific Instrument Company, but only about a dozen copies now remain. In the course of the preparation of the "guide sheets" of the method proposed in this paper, I found that there are many small mistakes in these Indian forms, but they are fortunately not of such a kind as to produce a sensible vitiation of results. I learn that the mistakes arose from a misunderstanding on the part of a computer employed to draw up the forms.

The accuracy of my guide sheets was controlled by aid of Mr. Roberts's forms, and it was the occasional discrepancy between my results and the forms which led to the detection of the errors referred to.

them to show how the strips are to be set. The guide sheet is laid on a drawing board, and the pins on the strips pierce the paper and fix them in their proper positions.

The shifting of the strips from one arrangement to the next is certainly slower than when they slid into their places automatically, but I find that even without practice it only takes about 7 or 8 minutes to shift 74 of them from any one arrangement to a new one.

The strip belonging to each mean solar day is divided by black lines into 24 equal spaces, intended for the entry of the hourly heights of water. The strip is 9 in. long by $\frac{1}{5}$ in. wide and the divisions ($\frac{3}{8}$ by $\frac{1}{5}$) are of convenient size for the entries. There was much difficulty in discovering a good material, but after various trials artificial ivory, or xylonite, was found to serve the purpose. Xylonite is white, will take writing with Indian ink or pencil, and can easily be cleaned with a damp cloth. It is just as easy to write with liquid Indian ink as with ordinary ink, which must not be used, because it stains the surface.

The strips have a great tendency to warp, and I have two methods of overcoming this. A veneer of xylonite on hard wood serves well, or solid xylonite may be stiffened by sheet brass let into a slot on the under side. In the first plan the pins are fixed in the wood, and in the second the brass is filed to a spike at each end. Whichever plan is adopted, the strips are expensive, costing about £7 for a set, and I do not at present see any way of making them cheaper.

The observations are to be treated in groups of two and a half lunations or 74 days. A set of strips, therefore, consists of 74, numbered from 0 to 73 in small figures on their flat ends.

If a set be pinned horizontally on a drawing board in vertical column, we have a form consisting of rows for each mean solar day and columns for each hour. The observed heights of the water are then written on the strips.

When the 24 columns are summed and divided by the number of entries we obtain the mean solar hourly mean heights. The harmonic analysis of these means gives the mean solar tides. But for evaluating the other tides the strips must be rearranged, and to this point we turn our attention.

Let us consider a special case, that of mean lunar time. A mean lunar hour is about $1^h\ 2^m$ m.s. time; hence the 12^h of each m.s. day must lie within 31^m m.s. time of a mean lunar hour. The following sample gives the incidence to the nearest lunar hour of the first few days in a year:—

Mean solar time.		Mean lunar time.
0^d 12^h	=	0^d 12^h
1 12	=	1 11
2 12	=	2 10
3 12	=	3 9
4 12	=	4 8
5 12	=	5 8
6 12	=	6 7
7 12	=	7 6
&c.		&c.

The successive 12^h of m.s. time will march retrogressively through all the 24 hours of m. lunar time.

Now, if starting from strip 0, we push strip 1 one division to the left, strip 2 two divisions to the left, and so on, the entries on the strips will be arranged in columns of approximately lunar time.

The rule for this arrangement is given by marks on a sheet of paper 18 in. broad; these marks consist of parallel numbered steps or zigzags showing where the ends of each strip are to be placed so as to bring the hourly values into their proper places.

At the end of a lunation mean solar time has gained a whole day over mean lunar time and the 12^h solar again agrees with the 12^h lunar. On the guide sheet we see that the zigzag which takes its origin at the left end of strip 0 has descended diagonally from right to left until it has reached the left margin of the paper, and a new zigzag then begins on the right margin.

When the strips are pinned out following the zigzags on the sheet marked M, the entries are arranged in 48 columns, but the number of entries in each column is different. The 48 columns are to be regarded as appertaining to $0^h, 1^h, \ldots, 22^h, 23^h, 0^h, 1^h, \ldots, 22^h, 23^h$. Thus, the number of entries in the left-hand column of any hour added to the number of entries in the right-hand column of the same hour is, in each case, 74. The 48 incomplete columns may, in fact, be regarded as 24 complete ones.

The 24 complete columns are then summed; the 24 sums would, if divided by the total number of strips, give the 24 mean lunar hourly heights. The harmonic analysis of these sums gives certain constants, which, when divided by the number of strips, are the required tidal constants. It must be remarked, however, that, as the incidence of the entries is not exact in lunar time, investigation must be made of the corrections arising out of this inexactness.

The explanation of the guide sheet for lunar time will serve, *mutatis mutandis*, for all the others.

The zigzags have to be placed so as to bring the columns into exact alignment, and printers' types provide all the accuracy requisite.

Accordingly, the computing strips are made to suit a chosen type. The standard length for one of the 24 divisions on the strips was chosen as that of a "2-em English quadrate"; 24 of these come to 9 inches, which is the length of a strip. I found the English quadrate a little too narrow, and accordingly between each line of quadrates there is a "blind rule," of 42 to the inch. The depth of the guide sheet is that of 74 quadrates and 74 rules, making $15\frac{1}{2}$ in. The computing strips are $\frac{1}{5}$ in. broad, and 74 of them occupy $14\frac{4}{5}$ in. The excess of $15\frac{1}{2}$ above $14\frac{4}{5}$, or $\frac{7}{10}$ in., is necessary to permit the easy arrangement of the strips.

To guard against the risk of the computer accidentally using the wrong sheet, the sheets are printed on coloured paper, the sequence of colours being that of the rainbow. The sheets for days 0 to 73 are all red; those for days 74 to 74+73, or 147, are all yellow; those for days 148 to 148+73, or 221, are green; those for days 222 to 222+73, or 295, are blue; and those for days 296 to 296+73, or 369, are violet.

Thus, when the observations for the first 74 days of the year are written on the strips all the sheets will be red; the strips will then be cleaned, and the observations for the second 74 days written in, when all the guide sheets will be yellow, and so on.

I must now refer to another considerable abridgment of the process of harmonic analysis. It is independent of the method of arrangement just sketched.

In the Indian computation forms the mean solar hourly heights have been found for the whole year, and the observations have been rearranged for the evaluation of certain other tides governed by a time scale which differs but little from the mean solar scale. I now propose to break the mean solar heights into sets of 30 days, and to analyse them, and next to harmonically analyse the 12 sets of harmonic constituents for annual and semi-annual inequalities. By this plan the harmonic constants for 11 different tides are obtained by one set of additions. In fact, we now get the annual, semi-annual, and solar elliptic tides, which formerly demanded much troublesome extra computation. A great saving is secured by this alone, and the results are in close agreement with those derived from the old method.

The guide sheets marked S and the computation forms are arranged so that the observations are broken up into the proper groups of 30 days, and they show the computer how to make the subsequent calculations.

I have also devised an abridged method of evaluating the tides of long period MSf, Mf, Mm. The method is less accurate than that followed hitherto, but it appears to give fairly good results, and reduces the work to very small dimensions.

Before entering on the details of my plan it is proper to mention that Dr. Börgen has devised and used a method for attaining the same end. He has prepared sheets of tracing paper with diagonal lines on them, so arranged that when any sheet is laid on the copy of the observations written in daily rows and hourly columns, the numbers to be summed are found written between a pair of lines. This plan is excellent, but I fear that the difficulty of adding correctly in diagonal lines is considerable, and the comparative faintness of figures seen through tracing paper may be fatiguing to the eyes. Dr. Börgen's plan is simple and inexpensive, and had I not thought that the plan now proposed has considerable advantages I should not have brought it forward.

In the investigations which follow the notation of the Report of 1883 to the British Association on harmonic analysis is used without further explanation.

§ 2. *Evaluation of* A_0, Sa, Ssa, S_1, S_2, S_4, S_6, T, R, K_2, K_1, P.

The 24 mean solar hourly heights of water are entered in a schedule of 24 columns, with one row for each day, extending to n days; the 24 columns are summed, and the sums divided by n; the 24 means are harmonically analysed; it is required to find from the results the values of the harmonic constituents.

The speed of any one of the tides differs from a multiple of 15° per hour by a small angle; thus, any one of the tides is expressible in the form $H\cos[(15^\circ q-\beta)t-\zeta]$, where q is 0, 1, 2, 3, &c., and β is small.

When t lies between 0^h and 24^h this formula expresses the oscillation of level due to this tide on the day 0 of the series of days.

If multiples of 24^h from 1 to $n-1$ be added to t, the expression gives the height at the same hour, t, of mean solar time on each of the succession of days.

Then if $\mathfrak{h}$ denotes the mean height of water, as due to this tide alone, at the hour t, we have

$$\mathfrak{h}=\frac{1}{n}\sum_1^n H\cos[(15^\circ q-\beta)\,t-\zeta+(15^\circ q-\beta)\,24\,(n-1)]$$

$$=\frac{1}{n}H\frac{\sin 12n\beta}{\sin\beta}\cos[(15^\circ q-\beta)t-\zeta-12\beta\,(n-1)]\ldots\ldots \quad (1).$$

When t is put successively equal to 0^h, 1^h, ..., 23^h we get the 24 values of $\mathfrak{h}$ which are to be submitted to harmonic analysis.

The mean value of $\mathfrak{h}$, say A_0 (not to be confused with A_0 as written at the head of this section, where is denoted the mean sea level above datum) is found by taking the mean of the 24 values of $\mathfrak{h}$.

By the formula for the summation of a series of cosines it is easy to prove that

$$A_o = \frac{1}{24n} H \frac{\sin 12n\beta}{\sin(\frac{1}{2}\beta - \frac{15}{2}q)} \cos\left[\zeta + (12n - \tfrac{1}{2}\beta) + \tfrac{15}{2}q\right] \quad \ldots \quad (2).$$

We will now find the p^{th} harmonic constituents A_p, B_p. By the ordinary rules

$$\left.\begin{matrix} A_p \\ B_p \end{matrix}\right\} = \tfrac{1}{12} \sum_0^{23} \mathfrak{h} \frac{\cos}{\sin} 15^\circ pt \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (3).$$

Now

$$2 \frac{\cos}{\sin} 15^\circ pt \cos\left[(15q - \beta)\, t - \zeta - 12\beta\,(n-1)\right]$$

$$= \frac{\cos}{\sin}\left[(15^\circ (q+p) - \beta)\, t - \zeta - 12\beta\,(n-1)\right]$$

$$\frac{+\cos}{-\sin}\left[(15^\circ (q-p) - \beta)\, t - \zeta - 12\beta\,(n-1)\right];$$

and $\frac{1}{12}$ of the sum of the 24 values corresponding to $t = 0, 1, \ldots, 23$ is

$$\tfrac{1}{24} \frac{\sin 12\left[15^\circ (q+p) - \beta\right]}{\sin\frac{1}{2}\left[15^\circ (q+p) - \beta\right]} \frac{\cos}{\sin}\left[\tfrac{23}{2}\left(15^\circ (q+p) - \beta\right) - \zeta - 12\beta\,(n-1)\right]$$

$\pm$ the same with sign of p changed

This expression admits of simplification, because $12 \times 15^\circ = 180^\circ$; making this simplification, and introducing the result into (3), we obtain

$$\left.\begin{matrix} A_p \\ B_p \end{matrix}\right\} = \frac{1}{24n} H \sin 12n\beta \left\{ \frac{\frac{-\cos}{+\sin}\left[\zeta + (12n - \frac{1}{2})\,\beta + \frac{15^\circ}{2}(q+p)\right]}{\sin\left[\frac{15^\circ}{2}(q+p) - \frac{1}{2}\beta\right]} \right.$$

$$\left. \frac{\frac{-\cos}{-\sin}\left[\zeta + (12n - \frac{1}{2})\,\beta + \frac{15^\circ}{2}(q-p)\right]}{\sin\left[\frac{15^\circ}{2}(q-p) - \frac{1}{2}\beta\right]} \right\} \ldots\ldots \quad (4).$$

In the particular case where $p = q$, we have

$$\left.\begin{matrix} A_q \\ B_q \end{matrix}\right\} = \frac{1}{24n} H \sin 12n\beta \left\{ \frac{\frac{-\cos}{+\sin}\left[\zeta + (12n - \frac{1}{2})\,\beta + 15^\circ q\right]}{\sin(15^\circ q - \frac{1}{2}\beta)} \right.$$

$$\left. \frac{\frac{+\cos}{+\sin}\left[\zeta + (12n - \frac{1}{2})\,\beta\right]}{\sin\frac{1}{2}\beta} \right\} \quad \ldots\ldots\ldots\ldots \quad (5).$$

If the number of days n be large, A_p, B_p will be small unless the denominator of one of the two terms in (4) be very small. This last case can only occur when $p = q$ and when β is small. Hence, in the analysis of a term of the form under consideration, we may neglect all the harmonics except the q^{th} one. Accordingly (2) and (5) are the only formulæ required.

A case, however, which there will be occasion to use hereafter is when $n = 30$, $q = 2$, when (4) becomes

$$\left.\begin{matrix} A_2 \\ B_2 \end{matrix}\right\} = \tfrac{1}{720} H \sin 360\beta \left\{ \frac{\begin{matrix}\cos\\ \sin\end{matrix}(\zeta + 359\tfrac{1}{2}\beta)}{\sin\tfrac{1}{2}\beta} \quad \frac{\begin{matrix}-\cos\\ +\sin\end{matrix}(\zeta + 30^\circ + 359\tfrac{1}{2}\beta)}{\sin(30^\circ - \tfrac{1}{2}\beta)} \right\} \quad (6).$$

For the present we have to apply (5) in the two cases $q = 1$, $\beta = 0^\circ{\cdot}0410686$ and $q = 2$, $\beta = 0^\circ{\cdot}0821372$; now the ratios of cosec $\tfrac{1}{2}\beta$ to cosec $(15^\circ q - \tfrac{1}{2}\beta)$ in these two cases are 722 to 1 and 697 to 1. In both cases the first term of (5) is negligible compared with the second.

Now write
$$\mathfrak{F} = \frac{24n \sin\tfrac{1}{2}\beta}{\sin 12n\beta} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (7),$$

and (5) becomes, with sufficient exactness,

$$\left.\begin{matrix} A_q \\ B_q \end{matrix}\right\} = \frac{H}{\mathfrak{F}} \begin{matrix}\cos\\ \sin\end{matrix} [\zeta + (12n - \tfrac{1}{2})\beta] \quad \ldots\ldots\ldots\ldots \quad (8).$$

If this be compared with (2), we see that when $q = 0$ this formula also comprises (2).

In the applications to be made β is very small, so that $\mathfrak{F}$ is approximately a function of the form θ cosec θ. This function increases very rapidly when θ passes 90°, but for considerable values less than 90° it only slightly exceeds unity; for example, when $\theta = 60^\circ$, $\mathfrak{F} = 1{\cdot}2$, but when $\theta = 180^\circ$, $\mathfrak{F} =$ infin.

It follows, therefore, that if the number n of days in the series is such that $12n\beta$ is less than say 60°, the magnitudes of A_q, B_q are but little diminished by division by $\mathfrak{F}$; but if $12n\beta$ is nearly 180°, A_q, B_q become vanishingly small.

If the typical tide here considered be the principal lunar tide M_2, and if the number of days be as nearly as possible an exact multiple of a semi-lunation, $12n\beta$ is nearly 180°, and the corresponding A_2, B_2 become very small. No number of whole days can be an exact multiple of a semi-lunation, so that A_2, B_2 corresponding to M_2 cannot be made to vanish completely. For the present they may be treated as negligible, and we return to this point in the next section.

The above investigation shows that in the expression for the whole

oscillation of sea level upon which the proposed analysis is performed all those tides may be omitted from which β is not very small, and also all those whose frequencies are such that the period under consideration $12n\beta$ is nearly 180°.

Since the period under consideration will be a lunation, it follows that, as far as is now material, the general expression for sea level may be written as follows, t denoting mean solar hour angle equal to $15°t$:—

m. w., annual	$A_0+H_{sa}\cos(h-\kappa_{sa})$
semi-annual	$+H_{ssa}\cos(2h-\kappa_{ssa})$
Solar tides, S_1, S_2........	$+H_{\frac{1}{2}s}\cos(t-\kappa_{\frac{1}{2}s})+H_s\cos(2t-\kappa_s)$
S_3, S_4........	$+H_{2s}\cos(4t-\kappa_{2s})+H_{3s}\cos(6t-\kappa_{3s})$
Solar elliptic, T	$+H_t\cos(2t-h+p_1-\kappa_t)$
R	$+H_r\cos(2t+h-p_1+\pi-\kappa_r)$
Luni-solar, K_2	$+f''H''\cos(2t+2h-2\nu''-\kappa'')$
K_1	$+f'H'\cos(t+h-\nu'-\frac{1}{2}\pi-\kappa')$
Solar diurnal, P	$+H_p\cos(t-h+\frac{1}{2}\pi-\kappa_p)$ .... (9).

This includes all the tides whose initials are written at the head of this section.

It is now necessary to break up the year into 12 equidistant lunations of 30 days. This can be done by the omission of 5 days in ordinary years, and of 6 days in leap years.

If the days of the year are numbered 0 to 364 (365 in leap year), the twelve months are as follows:—

0, 0^d to 29^d; 1, 30^d to 59^d; *omit* 60^d; 2, 61^d to 90^d; 3, 91^d to 120^d; *omit* 121^d; 4, 122^d to 151^d; 5, 152^d to 181^d; *omit* 182^d; 6, 183^d to 212^d; 7, 213^d to 242; 8, 243^d to 272^d; *omit* 273^d; 9, 274^d to 303^d; 10, 304^d to 333^d; *omit* 334^d; 11, 335^d to 364^d; *in leap year omit* 365^d.

The increments of sun's mean longitude from $0^d\ 0^h$ of month 0 up to 0^h of the day numbered 0 of each group of days or month are as follows:—

0, 0°; 1, 30°−0°·431; 2, 60°·124; 3, 90°−0°·306; 4, 120°·249; 5, 150°−0°·182; 6, 180°·373; 7, 210°−0°·057; 8, 240°−0°·488; 9, 270°·068; 10, 300°−0°·364; 11, 330°·191.

Thus if h_0 be the sun's mean longitude at $0^d\ 0^h$ of month 0, the sun's mean longitude at $0^d\ 0^h$ of month τ is $h_0+30°\tau$, with sufficient approximation.

Now let V with appropriate suffix denote the initial "equilibrium argument" at $0^d\ 0^h$ of month 0, so that

$$V_{sa} = h_0,\ V_{ssa} = 2h_0,\ V_t = -h_0+p_1,\ V_r = h_0-p_1+\pi,\ V'' = 2h_0-2\nu'',$$

$$V' = h_0-\nu'-\tfrac{1}{2}\pi,\ V_p = -h_0+\tfrac{1}{2}\pi;$$

then the general expression (9) for the tide in the month τ becomes

$$\begin{aligned}
&A_0+H_{sa}\cos(\eta t+V_{sa}+30^\circ\tau-\kappa_{sa})+H_{ssa}\cos(2\eta t+V_{ssa}+60^\circ\tau-\kappa_{ssa})\\
&+H_{\frac{1}{2}s}\cos(15^\circ t-\kappa_{\frac{1}{2}s})+H_s\cos(30^\circ t-\kappa_s)+H_{2s}\cos(60^\circ t-\kappa_{2s})\\
&\qquad\qquad+H_{3s}\cos(90^\circ t-\kappa_{3s})\\
&+H_t\cos[(30^\circ-\eta)t+V_t-30^\circ\tau-\kappa_t]\\
&\qquad\qquad+H_r\cos[(30^\circ+\eta)t+V_r+30^\circ\tau-\kappa_r]\\
&+f''H''\cos[(30^\circ+2\eta)t+V''+60^\circ\tau-\kappa'']\\
&\qquad\qquad+f'H'\cos[15^\circ+\eta)t+V'+30^\circ\tau-\kappa']\\
&+H_p\cos[(15^\circ-\eta)t+V_p-30^\circ\tau-\kappa_p]\ \ldots\ldots\ldots\ldots\ldots\ldots\ (10).
\end{aligned}$$

Each of these terms falls into the type $\cos[(15^\circ q-\beta)t-\zeta]$, and β is in every case either $\pm\eta, -2\eta$, or 0.

Now, when harmonic analysis of the mean of 30 days is carried out, coefficients $\mathfrak{F}$ are introduced.

Write therefore

$$\mathfrak{F}_1 = \frac{24\times 30\sin\frac{1}{2}\eta}{\sin 360\eta},\qquad \mathfrak{F}_2 = \frac{24\times 30\sin\eta}{\sin 720\eta}.$$

With the known value of η,

$$\log\mathfrak{F}_1 = 0{\cdot}00483,\qquad \log\mathfrak{F}_2 = 0{\cdot}01945.$$

In applying the method investigated above, it will be observed that a term of any frequency $15^\circ q-\beta$ only contributes to the harmonic constituent of order q.

Then applying our general rule (8) term by term, and observing that $359\frac{1}{2}\eta = 14^\circ{\cdot}76$, and $719\eta = 29^\circ{\cdot}53$, the result may be written as follows:—

$$\mathfrak{A}_0^{(\tau)} = A_0+\frac{H_{sa}}{\mathfrak{F}_1}\cos(\kappa_{sa}-V_{sa}-30^\circ\tau-14^\circ{\cdot}76)$$
$$\qquad\qquad+\frac{H_{ssa}}{\mathfrak{F}_2}\cos(\kappa_{ssa}-V_{ssa}-60^\circ\tau-29^\circ{\cdot}53);$$

$$\left.\begin{matrix}\mathfrak{A}_1^{(\tau)}\\ \mathfrak{B}_1^{(\tau)}\end{matrix}\right\} = H_{\frac{1}{2}s}\begin{matrix}\cos\\ \sin\end{matrix}\kappa_{\frac{1}{2}s}+\frac{f'H'}{\mathfrak{F}_1}\begin{matrix}\cos\\ \sin\end{matrix}(\kappa'-V'-30^\circ\tau-14^\circ{\cdot}76)$$
$$\qquad\qquad+\frac{H_p}{\mathfrak{F}_1}\begin{matrix}\cos\\ \sin\end{matrix}(\kappa_p-V_p+30^\circ\tau+14^\circ{\cdot}76);$$

$$\left.\begin{matrix}\mathfrak{A}_2^{(\tau)}\\ \mathfrak{B}_2^{(\tau)}\end{matrix}\right\} = H_s\,\frac{\cos}{\sin}\,\kappa_s + \frac{H_t}{\mathfrak{f}_1}\frac{\cos}{\sin}\,(\kappa_t - \mathrm{V}_t + 30^\circ\tau + 14^\circ\!\cdot\!76)$$
$$+\frac{H_r}{\mathfrak{f}_1}\frac{\cos}{\sin}\,(\kappa_r - \mathrm{V}_r - 30^\circ\tau - 14^\circ\!\cdot\!76)$$
$$+\frac{\mathrm{f}''H''}{\mathfrak{f}_2}\frac{\cos}{\sin}\,(\kappa'' - \mathrm{V}'' - 60^\circ\tau - 29^\circ\!\cdot\!53);$$

$$\left.\begin{matrix}\mathfrak{A}_4^{(\tau)}\\ \mathfrak{B}_4^{(\tau)}\end{matrix}\right\} = H_{2s}\,\frac{\cos}{\sin}\,\kappa_{2s}; \qquad \left.\begin{matrix}\mathfrak{A}_6^{(\tau)}\\ \mathfrak{B}_6^{(\tau)}\end{matrix}\right\} = H_{3s}\,\frac{\cos}{\sin}\,\kappa_{3s}. \;\ldots\ldots\ldots\; (11).$$

With the meaning of the term month in the present context, the sun has a mean motion of 30° per month, and each of the first five $\mathfrak{A}$'s and $\mathfrak{B}$'s is a function with a constant part and with annual and semi-annual inequalities.

When τ has successively the 12 values 0, 1, ..., 11, we have 12 equidistant values of the $\mathfrak{A}$'s and $\mathfrak{B}$'s. These may be harmonically analysed for annual and semi-annual inequalities.

Suppose that the several coefficients to be determined by harmonic analysis are defined by the following equations :—

$$\mathfrak{A}_0^{(\tau)} = A_0 + A_1\cos 30^\circ\tau + B_1\sin 30^\circ\tau + A_2\cos 60^\circ\tau + B_2\sin 60^\circ\tau;$$

$$\left.\begin{matrix}\mathfrak{A}_1^{(\tau)}\\ \mathfrak{B}_1^{(\tau)}\\ \mathfrak{A}_2^{(\tau)}\\ \mathfrak{B}_2^{(\tau)}\end{matrix}\right\} = \left.\begin{matrix}\mathrm{C}_0\\ \mathrm{c}_0\\ \mathrm{E}_0\\ \mathrm{e}_0\end{matrix}\right\} + \left.\begin{matrix}\mathrm{C}_1\\ \mathrm{c}_1\\ \mathrm{E}_1\\ \mathrm{e}_1\end{matrix}\right\}\cos 30^\circ\tau + \left.\begin{matrix}\mathrm{D}_1\\ \mathrm{d}_1\\ \mathrm{F}_1\\ \mathrm{f}_1\end{matrix}\right\}\sin 30^\circ\tau + \left.\begin{matrix}\\ \\ \mathrm{E}_2\\ \mathrm{e}_2\end{matrix}\right\}\cos 60^\circ\tau + \left.\begin{matrix}\\ \\ \mathrm{F}_2\\ \mathrm{f}_2\end{matrix}\right\}\sin 60^\circ\tau$$

$$\tfrac{1}{12}\Sigma\mathfrak{A}_4^{(\tau)} = A_4,\; \tfrac{1}{12}\Sigma\mathfrak{A}_4^{(\tau)} = B_4,\; \tfrac{1}{12}\Sigma\mathfrak{A}_6^{(\tau)} = A_6,\; \tfrac{1}{12}\Sigma\mathfrak{B}_6^{(\tau)} = B_6 \,.\,.\; (12).$$

Then on comparing (12) with (11) we see that :—

$$A_0 = A_0;$$

$$\left.\begin{matrix}A_1\\ B_1\end{matrix}\right\} = \frac{H_{sa}}{\mathfrak{f}_1}\frac{\cos}{\sin}\,(\kappa_{sa} - \mathrm{V}_{sa} - 14^\circ\!\cdot\!76),\; \left.\begin{matrix}A_2\\ B_2\end{matrix}\right\} = \frac{H_{ssa}}{\mathfrak{f}_2}\frac{\cos}{\sin}\,(\kappa_{ssa} - \mathrm{V}_{ssa} - 29^\circ\!\cdot\!53);$$

$$\left.\begin{matrix}\mathrm{C}_0\\ \mathrm{c}_0\end{matrix}\right\} = H_{\frac{1}{2}s}\,\frac{\cos}{\sin}\,\kappa_{\frac{1}{2}s},$$

$$\left.\begin{matrix}\mathrm{C}_1\\ \mathrm{D}_1\end{matrix}\right\} = \begin{matrix}+\\ +\end{matrix}\frac{\mathrm{f}'H'}{\mathfrak{f}_1}\frac{\cos}{\sin}\,(\kappa' - \mathrm{V}' - 14^\circ\!\cdot\!76)\; \begin{matrix}+\\ -\end{matrix}\frac{H_p}{\mathfrak{f}_1}\frac{\cos}{\sin}\,(\kappa_p - \mathrm{V}_p + 14^\circ\!\cdot\!76);$$

$$\left.\begin{matrix}\mathrm{c}_1\\ \mathrm{d}_1\end{matrix}\right\} = \begin{matrix}+\\ -\end{matrix}\frac{\mathrm{f}'H'}{\mathfrak{f}_1}\frac{\sin}{\cos}\,(\kappa' - \mathrm{V}' - 14^\circ\!\cdot\!76)\; \begin{matrix}+\\ +\end{matrix}\frac{H_p}{\mathfrak{f}_1}\frac{\sin}{\cos}\,(\kappa_p - \mathrm{V}_p + 14^\circ\!\cdot\!76);$$

$$\left.\begin{matrix}\mathrm{E}_0\\ \mathrm{e}_0\end{matrix}\right\} = H_s\,\frac{\cos}{\sin}\,\kappa_s,$$

$$\left.\begin{matrix}E_1\\F_1\end{matrix}\right\} = \frac{+H_t}{-\mathfrak{f}_1}\begin{matrix}\cos\\\sin\end{matrix}(\kappa_t - V_t + 14^\circ{\cdot}76) \frac{+H_r}{+\mathfrak{f}_1}\begin{matrix}\cos\\\sin\end{matrix}(\kappa_r - V_r - 14^\circ{\cdot}76);$$

$$\left.\begin{matrix}e_1\\f_1\end{matrix}\right\} = \frac{+H_t}{+\mathfrak{f}_1}\begin{matrix}\sin\\\cos\end{matrix}(\kappa_t - V_t + 14^\circ{\cdot}76) \frac{+H_r}{-\mathfrak{f}_1}\begin{matrix}\sin\\\cos\end{matrix}(\kappa_r - V_r - 14^\circ{\cdot}76);$$

$$\left.\begin{matrix}E_2\\F_2\end{matrix}\right\} = \frac{+f''H''}{+\mathfrak{f}_2}\begin{matrix}\cos\\\sin\end{matrix}(\kappa'' - V'' - 29^\circ{\cdot}53);$$

$$\left.\begin{matrix}e_2\\f_2\end{matrix}\right\} = \frac{+f''H''}{-\mathfrak{f}_2}\begin{matrix}\sin\\\cos\end{matrix}(\kappa'' - V'' - 29^\circ{\cdot}53) \quad\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (13).$$

From these equations we get

$$\left.\begin{matrix}\frac{1}{2}(C_1 - d_1)\\\frac{1}{2}(c_1 + D_1)\end{matrix}\right\} = \frac{f'H'}{\mathfrak{f}_1}\begin{matrix}\cos\\\sin\end{matrix}(\kappa' - V' - 14^\circ{\cdot}76);$$

$$\left.\begin{matrix}\frac{1}{2}(C_1 + d_1)\\\frac{1}{2}(c_1 - D_1)\end{matrix}\right\} = \frac{H_p}{\mathfrak{f}_1}\begin{matrix}\cos\\\sin\end{matrix}(\kappa_p - V_p + 14^\circ{\cdot}76);$$

$$\left.\begin{matrix}\frac{1}{2}(E_1 - f_1)\\\frac{1}{2}(e_1 + F_1)\end{matrix}\right\} = \frac{H_r}{\mathfrak{f}_1}\begin{matrix}\cos\\\sin\end{matrix}(\kappa_r - V_r - 14^\circ{\cdot}76);$$

$$\left.\begin{matrix}\frac{1}{2}(E_1 + f_1)\\\frac{1}{2}(e_1 - F_1)\end{matrix}\right\} = \frac{H_t}{\mathfrak{f}_1}\begin{matrix}\cos\\\sin\end{matrix}(\kappa_t - V_t + 14^\circ{\cdot}76);$$

$$\left.\begin{matrix}\frac{1}{2}(E_2 - f_2)\\\frac{1}{2}(e_2 + F_2)\end{matrix}\right\} = \frac{f''H''}{\mathfrak{f}_2}\begin{matrix}\cos\\\sin\end{matrix}(\kappa'' - V'' - 29^\circ{\cdot}53)\ldots\ldots \quad (14).$$

The tidal constants of the several tides enumerated at the head of this section are determinable from these equations.

Our rule is accordingly to analyse in twelve groups of 30 days, and then to analyse the resulting harmonic constituents for annual and semi-annual inequalities, combining the final results according to the formula just found.

The edition of "guide sheets" and computation forms which I have drawn up are so arranged as to facilitate the whole process and to render it quite straightforward. By this single set of additions it is thus possible to evaluate eleven tides and mean water.

§ 3. *Clearance of* T, R *from perturbation by* M_2.

The method of the last section was designed to render all the tides insensible excepting those enumerated. But M_2 is so much larger than any other tide that there is a small residual disturbance which ought to be corrected.

I have made computations, which I do not give, but which show that the disturbance of all the harmonic constituents except $\mathfrak{A}_2$, $\mathfrak{B}_2$

is insensible. It is required then to determine the correction to be applied to $\mathfrak{A}_2$, $\mathfrak{B}_2$, and thence those for E, F, e, f.

Suppose that, when time t is counted from $0^d\ 0^h$ of month τ, the M_2 tide is expressed by $M\cos[(30^\circ-\beta)t-\zeta]$, where $\beta = 1^\circ{\cdot}0158958$.

When means taken over 30 days are harmonically analysed the formula (6) gives the contributions to $\mathfrak{A}_2$, $\mathfrak{B}_2$. As it is now required to obliterate these contributions, the signs must be changed, and the corrections are

$$\left.\begin{array}{l}\delta\mathfrak{A}_2^{(\tau)}\\ \delta\mathfrak{B}_2^{(\tau)}\end{array}\right\} = -\tfrac{1}{720}M\sin 360\,\beta\left\{\frac{\begin{array}{c}\cos\\ \sin\end{array}(\zeta+359\tfrac{1}{2}\beta)}{\sin\tfrac{1}{2}\beta}\ \frac{\begin{array}{c}-\cos\\ +\sin\end{array}(\zeta+30^\circ+359\tfrac{1}{2}\beta)}{\sin(30^\circ-\tfrac{1}{2}\beta)}\right\} \quad \ldots\ldots \ (15).$$

For reasons which will appear below I now write

$$\zeta = \zeta_m^{(\tau)} - 0^\circ{\cdot}5258.$$

Then introducing the value of β into (15), I find

$$\left.\begin{array}{l}\delta\mathfrak{A}_2^{(\tau)}\\ \delta\mathfrak{B}_2^{(\tau)}\end{array}\right\} = -\tfrac{1}{720}M\sin 5^\circ\,43'{\cdot}35\left\{\frac{\begin{array}{c}\cos\\ \sin\end{array}(\zeta_m^{(\tau)}+4^\circ{\cdot}689)}{\sin 0^\circ\,30'\,28''{\cdot}6}\ \frac{\begin{array}{c}-\cos\\ +\sin\end{array}(\zeta_m^{(\tau)}+34^\circ{\cdot}689)}{\sin 29^\circ\,29'{\cdot}52}\right\} \ \ldots\ (16).$$

Let ζ_m denote the value of $\zeta_m^{(\tau)}$ at $0^d\ 0^h$ of month 0, and let $\zeta_m^{(\tau)} = \zeta_m + \theta^{(\tau)}$, and let $\mathfrak{f}_2$ denote a certain factor whose logarithm is 0·00849, and let $M = \mathrm{f}H_m$.

In the harmonic analysis for the M_2 tide, considered below in § 6, we shall have

$$A_2 = \frac{\mathrm{f}H_m}{\mathfrak{f}_2}\cos\zeta_m, \qquad B_2 = \frac{\mathrm{f}H_m}{\mathfrak{f}_2}\sin\zeta_m.$$

Accordingly

$$M\begin{array}{c}\cos\\ \sin\end{array}\zeta_m^{(\tau)} = \mathfrak{f}_2\left[A_2\begin{array}{c}\cos\\ \sin\end{array}\theta^{(\tau)}\mp B_2\begin{array}{c}\sin\\ \cos\end{array}\theta^{(\tau)}\right].$$

These values of $M\begin{array}{c}\cos\\ \sin\end{array}\zeta_m^{(\tau)}$ must now be introduced into (16), but the algebraic process need not be given in detail. If we write

$$\left.\begin{array}{l}\tfrac{1}{2}(\mathrm{S}+\mathrm{P})\\ \tfrac{1}{2}(\mathrm{R}+\mathrm{Q})\end{array}\right\} = \tfrac{1}{720}\mathfrak{f}_2\,\frac{\sin 5^\circ\,43'{\cdot}35\begin{array}{c}\cos\\ \sin\end{array}(4^\circ\,41'{\cdot}32)}{\sin 0^\circ\,30'\,28''{\cdot}6},$$

$$\left.\begin{array}{l}\tfrac{1}{2}(\mathrm{S}-\mathrm{P})\\ \tfrac{1}{2}(\mathrm{R}-\mathrm{Q})\end{array}\right\} = \tfrac{1}{720}\mathfrak{f}_2\,\frac{\sin 5^\circ\,43'{\cdot}35\begin{array}{c}\cos\\ \sin\end{array}(34^\circ\,41'{\cdot}32)}{\sin 29^\circ\,29'\,31''{\cdot}4},$$

it follows that

$$P = 0{\cdot}01564, \quad Q = 0{\cdot}00114, \quad R = 0{\cdot}00147, \quad S = 0{\cdot}01611.$$

Then, when the substitution of the values of $M \frac{\sin}{\cos} \zeta_m^{(\tau)}$ is carried out, we find

$$\left.\begin{matrix}\delta\mathfrak{A}_2^{(\tau)} \\ \delta\mathfrak{B}_2^{(\tau)}\end{matrix}\right\} = \cos\theta^{(\tau)}\left[\begin{matrix}-P \\ -R\end{matrix} A_2 \begin{matrix}+Q \\ -S\end{matrix} B_2\right] + \sin\theta^{(\tau)}\left[\begin{matrix}+Q \\ -S\end{matrix} A_2 \begin{matrix}+P \\ +R\end{matrix} B_2\right].$$

By the definition of $\theta^{(\tau)}$ it appears that $-\theta^{(\tau)}$ is the increment of twice the mean moon's hour angle during the time from $0^d\ 0^h$ of month 0 up to $0^d\ 0^h$ of month τ, that is to say $\theta^{(\tau)} = -2(\gamma - \sigma)t$ for the time specified. The following table gives the values of $\theta^{(\tau)}$ and of its cosine and sine for each month :—

Month (τ).	No. of days from epoch 0 to epoch τ.	$\theta^{(\tau)}$.	$\cos\theta^{(\tau)}$.	$\sin\theta^{(\tau)}$.
0	0	0° 0′	1·000	0·000
1	30	11 27	0·980	0·199
2	61	47 16	0·678	0·735
3	91	58 43	0·519	0·855
4	122	π−85 28	−0·079	0·997
5	152	π−74 1	−0·275	0·961
6	183	π−38 11	−0·786	0·618
7	213	π−26 44	−0·893	0·450
8	243	π−15 18	−0·965	0·264
9	274	π+20 32	−0·937	−0·351
10	304	π+31 59	−0·848	−0·530
11	335	π+67 49	−0·378	−0·926

If $\cos\theta^{(\tau)}$, $\sin\theta^{(\tau)}$ are regarded as quantities having annual and semi-annual inequalities, we may write

$$\cos\theta^{(\tau)} = \alpha_0 + \alpha_1\cos 30^\circ\tau + \beta_1\sin 30^\circ\tau + \alpha_2\cos 60^\circ\tau + \beta_2\sin 60^\circ\tau + \ldots.$$

$$\sin\theta^{(\tau)} = \gamma_0 + \gamma_1\cos 30^\circ\tau + \delta_1\sin 30^\circ\tau + \gamma_2\cos 60^\circ\tau + \delta_2\sin 60^\circ\tau + \ldots.$$

On analysing the numerical values of $\cos\theta^{(\tau)}$, $\sin\theta^{(\tau)}$ by the ordinary processes, I find,

$$\begin{aligned} \alpha_0 &= -0{\cdot}165, & \gamma_0 &= +0{\cdot}273, \\ \alpha_1 &= +0{\cdot}626, & \gamma_1 &= -0{\cdot}500, \\ \beta_1 &= +0{\cdot}756, & \delta_1 &= +0{\cdot}642, \\ \alpha_2 &= +0{\cdot}159, & \gamma_2 &= -0{\cdot}046, \\ \beta_2 &= +0{\cdot}199, & \delta_2 &= +0{\cdot}166. \end{aligned}$$

But in § 2 the harmonic constituents of $\mathfrak{A}_2$ when analysed for

annual and semi-annual inequality were denoted by E_0, E_1, F_1, E_2, F_2, and the constituents of $\mathfrak{B}_2$ were denoted by e_0, e_1, f_1, e_2, f_2. Hence the ten corrections to the E's and F's are (with an easily intelligible alternative notation)

$$\delta E_{0,1,2} = (-P\alpha_{0,1,2} + Q\gamma_{0,1,2})\,A_2 + (Q\alpha_{0,1,2} + P\gamma_{0,1,2})\,B_2,$$

$$\delta F_{1,2} = (-P\beta_{1,2} + Q\delta_{1,2})\,A_2 + (Q\beta_{1,2} + P\delta_{1,2})\,B_2,$$

$$\delta e_{0,1,2} = (-R\alpha_{0,1,2} - S\gamma_{0,1,2})\,A_2 + (-S\alpha_{0,1,2} + R\gamma_{0,1,2})\,B_2,$$

$$\delta f_{1,2} = (-R\beta_{1,2} - S\delta_{1,2})\,A_2 + (-S\beta_{1,2} + R\delta_{1,2})\,B_2.$$

On substituting the numerical values of α, β, γ, δ, P, Q, R, S, I find

	Coeffit. of A_2.	Coeffit. of B_2.
$\delta E_0 =$	$+0{\cdot}0029$	$+0{\cdot}0041$
$\delta e_0 =$	$-0{\cdot}0042$	$+0{\cdot}0031$
$\delta\frac{1}{2}(E_1+f_1) =$	$-0{\cdot}0109$	$-0{\cdot}0091$
$\delta\frac{1}{2}(E_1-f_1) =$	$+0{\cdot}0006$	$+0{\cdot}0020$
$\delta\frac{1}{2}(e_1+F_1) =$	$-0{\cdot}0020$	$+0{\cdot}0000$
$\delta\frac{1}{2}(e_1-F_1) =$	$+0{\cdot}0091$	$-0{\cdot}0108$
$\delta\frac{1}{2}(E_2+f_2) =$	$-0{\cdot}0028$	$-0{\cdot}0018$
$\delta\frac{1}{2}(E_2-f_2) =$	$+0{\cdot}0002$	$+0{\cdot}0012$
$\delta\frac{1}{2}(e_2+F_2) =$	$-0{\cdot}0012$	$+0{\cdot}0001$
$\delta\frac{1}{2}(e_2-F_2) =$	$+0{\cdot}0017$	$-0{\cdot}0027$

Most of these corrections are negligible, but the four which affect the solar elliptic tides T, R must be included, because those tides are so small that a small error affects them sensibly. Hence we may take, with sufficient accuracy,

$$\delta\tfrac{1}{2}(e_1-F_1) = +0{\cdot}009\,A_2 - 0{\cdot}011\,B_2, \quad \delta\tfrac{1}{2}(e_1+F_1) = -0{\cdot}002\,A_2,$$

$$\delta\tfrac{1}{2}(E_1+f_1) = -0{\cdot}011\,A_2 - 0{\cdot}009\,B_2, \quad \delta\tfrac{1}{2}(E_1-f_1) = +0{\cdot}0006\,A_2 + 0{\cdot}002\,B_2 \quad \ldots\ldots \ (16^*),$$

where A_2, B_2 are the components of the M_2 derived from the reduction of that tide by the process of § 6.

Provision for these corrections is made in the computation forms.

§ 4. *Evaluation of* A_0, Sa, S_1, S_2, S_4, S_6, K_2, K_1, P, *when a complete year of observation is not available.*

It is now proposed to consider the case where the period of observation is as much as six complete months and less than a complete year.

The method of the last section apparently depends on the completeness of the year, yet, with certain modifications, it may be rendered available for shorter periods.

We suppose that so much of the year as is available is broken into sets of 30 days by the rules of the last section, and that the means are harmonically analysed. The results of such harmonic analysis for month (τ) are given in (11) of § 3, but for the purpose in hand they now admit of some simplification. It is clear that it is not worth while to evaluate the very small solar elliptic tides T and R from a short period of observation. If, then, we denote by $P^{(\tau)}$ the ratio of the cube of the sun's parallax to its mean parallax at the middle of the month (τ), the first three terms of the third of (11) may be included in the expression $P^{(\tau)}H_s \frac{\cos}{\sin} \kappa_s$. The last term of this equation really does involve the solar parallax to some extent, and we may, with sufficient approximation, write the third pair of equations

$$\left.\begin{matrix}\mathfrak{A}_2^{(\tau)} \div P^{(\tau)} \\ \mathfrak{B}_2^{(\tau)} \div P^{(\tau)}\end{matrix}\right\} = H_s \frac{\cos}{\sin} \kappa_s + \frac{f''H''}{\mathfrak{f}_2} \frac{\cos}{\sin} (\kappa'' - V'' - 60^\circ\tau - 29^\circ{\cdot}53).$$

Let us now consider the value of $P^{(\tau)}$. The longitude of the solar perigee is 281° or −79°, and the ratio of the sun's parallax to its mean parallax is approximately $1 + e_1 \cos(h + 79^\circ)$, and the cube of that ratio is $1 + 3e_1 \cos(h + 79^\circ)$ or $1 + 0{\cdot}0504 \cos(h + 79^\circ)$. Now h, the sun's longitude at the middle of month (τ), is $h_0 + 15^\circ + 30^\circ\tau$; hence

$$P^{(\tau)} = 1 + 0{\cdot}0504 \cos(h_0 + 30^\circ\tau + 94^\circ)$$

and
$$\frac{1}{P^{(\tau)}} = 1 - 0{\cdot}0504 \cos(h_0 + 30^\circ\tau + 94^\circ).$$

Thus it is easy to compute the values of $1/P^{(\tau)}$ for the successive months, when we know h_0 the sun's mean longitude at $0^d\ 0^h$ of the month 0.

The semi-annual tide, being usually small, may be neglected in these incomplete observations, and the equations (11) now become

$$\mathfrak{A}_0^{(\tau)} = A_0 + \frac{H_{sa}}{\mathfrak{f}_1} \cos(\kappa_{sa} - V_{sa} - 30^\circ\tau - 14^\circ{\cdot}76),$$

$$\left.\begin{matrix}\mathfrak{A}_1^{(\tau)} \\ \mathfrak{B}_1^{(\tau)}\end{matrix}\right\} = H_{\mu} \frac{\cos}{\sin} \kappa_{\mu} + \frac{f'H'}{\mathfrak{f}_1} \frac{\cos}{\sin} (\kappa' - V' - 30^\circ\tau - 14^\circ{\cdot}76)$$
$$+ \frac{H_p}{\mathfrak{f}_1} \frac{\cos}{\sin} (\kappa_p - V_p + 30^\circ\tau + 14^\circ{\cdot}76),$$

$$\left.\begin{matrix}\mathfrak{A}_2^{(\tau)} \div P^{(\tau)} \\ \mathfrak{B}_2^{(\tau)} \div P^{(\tau)}\end{matrix}\right\} = H_s \frac{\cos}{\sin} \kappa_s + \frac{f''H''}{\mathfrak{f}_2} \frac{\cos}{\sin} (\kappa'' - V'' - 60^\circ\tau - 29^\circ{\cdot}53),$$

$$\left.\begin{matrix}\mathfrak{A}_4^{(\tau)}\\ \mathfrak{B}_4^{(\tau)}\end{matrix}\right\} = H_{2s}\begin{matrix}\cos\\ \sin\end{matrix}\kappa_{2s},$$

$$\left.\begin{matrix}\mathfrak{A}_6^{(\tau)}\\ \mathfrak{B}_6^{(\tau)}\end{matrix}\right\} = H_{3s}\begin{matrix}\cos\\ \sin\end{matrix}\kappa_{3s}, \qquad \frac{1}{P^{(\tau)}} = 1-0{\cdot}0504\cos(h_0+30^\circ\tau+94^\circ) \quad \ldots\ldots \quad (17).$$

When the series of successive values of the $\mathfrak{A}$'s and $\mathfrak{B}$'s are harmonically analysed (by processes which we shall consider shortly) the several coefficients resulting from such analysis will be defined by

$$\mathfrak{A}_0^{(\tau)} = A_0+A_1\cos 30^\circ\tau+B_1\sin 30^\circ\tau,$$

$$\left.\begin{matrix}\mathfrak{A}_1^{(\tau)}\\ \mathfrak{B}_1^{(\tau)}\end{matrix}\right\} = \left.\begin{matrix}\mathrm{C}_0\\ \mathrm{c}_0\end{matrix}\right\} + \left.\begin{matrix}\mathrm{C}_1\\ \mathrm{c}_1\end{matrix}\right\}\cos 30^\circ\tau + \left.\begin{matrix}\mathrm{D}_1\\ \mathrm{d}_1\end{matrix}\right\}\sin 30^\circ\tau,$$

$$\left.\begin{matrix}\mathfrak{A}_2^{(\tau)}\div P^{(\tau)}\\ \mathfrak{B}_2^{(\tau)}\div P^{(\tau)}\end{matrix}\right\} = \left.\begin{matrix}\mathrm{E}_0\\ \mathrm{e}_0\end{matrix}\right\} + \left.\begin{matrix}\mathrm{E}_2\\ \mathrm{e}_2\end{matrix}\right\}\cos 60^\circ\tau + \left.\begin{matrix}\mathrm{F}_2\\ \mathrm{f}_2\end{matrix}\right\}\sin 60^\circ\tau,$$

$$\text{Mean } \mathfrak{A}_4^{(\tau)} = A_4, \qquad \text{Mean } \mathfrak{B}_4^{(\tau)} = B_4,$$

$$\text{Mean } \mathfrak{A}_6^{(\tau)} = A_6, \qquad \text{Mean } \mathfrak{B}_6^{(\tau)} = B_6 \quad \ldots\ldots \quad (18).$$

Then the subsequent procedure as given in (13) and (14) holds good, the only difference being that we do not obtain the semi-annual and solar elliptic tides.

We shall now consider the harmonic analysis of an imperfect series of values.

It must be premised that each monthly value of $\mathfrak{A}_2^{(\tau)}$, $\mathfrak{B}_2^{(\tau)}$ is to be divided by its corresponding $P^{(\tau)}$ before the analysis is made.

Suppose that $C^{(\tau)}$ denotes a function which is subject to semi-annual inequality, and that

$$C^{(\tau)} = \mathrm{A}_0+\mathrm{A}_2\cos 60^\circ\tau+\mathrm{B}_2\sin 60^\circ\tau$$

Then it is clear that

$$C^{(0)} = \mathrm{A}_0+\mathrm{A}_2,$$

$$C^{(1)} = \mathrm{A}_0+\tfrac{1}{2}\mathrm{A}_2+\tfrac{1}{2}\sqrt{3}\,\mathrm{B}_2,$$

$$C^{(2)} = \mathrm{A}_0-\tfrac{1}{2}\mathrm{A}_2+\tfrac{1}{2}\sqrt{3}\,\mathrm{B}_2,$$

&c. &c.

I now define D_0, D_1, D_2 thus:—

$$D_0 = C^{(0)}+C^{(1)}+C^{(2)}+\ldots\ldots,$$

$$D_1 = C^{(0)}+\tfrac{1}{2}C^{(1)}-\tfrac{1}{2}C^{(2)}\ldots\ldots,$$

$$D_2 = 0\,.\,C^{(0)}+\tfrac{1}{2}\sqrt{3}\,C^{(1)}+\tfrac{1}{2}\sqrt{3}\,C^{(2)}\ldots\ldots$$

If there be n equations and if they be treated by the method of least squares, we get

$$D_0 = n\mathrm{A}_0 + \mathrm{A}_2(1+\tfrac{1}{2}-\tfrac{1}{2}\ldots\ldots) + \mathrm{B}_2(0+\tfrac{1}{2}\sqrt{3}+\tfrac{1}{2}\sqrt{3}\ldots\ldots),$$

$$D_1 = \mathrm{A}_0(1+\tfrac{1}{2}-\tfrac{1}{2}\ldots\ldots) + \mathrm{A}_2(1+\tfrac{1}{4}+\tfrac{1}{4}\ldots\ldots) + \mathrm{B}_0(0+\tfrac{1}{4}\sqrt{3}-\tfrac{1}{4}\sqrt{3}\ldots\ldots),$$

$$D_2 = \mathrm{A}_0(0+\tfrac{1}{2}\sqrt{3}+\tfrac{1}{2}\sqrt{3}\ldots\ldots) + \mathrm{A}_2(0+\tfrac{1}{4}\sqrt{3}-\tfrac{1}{4}\sqrt{3}\ldots\ldots)$$
$$+\mathrm{B}_0(0+\tfrac{3}{4}+\tfrac{3}{4}\ldots\ldots).$$

These are the three equations from which A_0, A_2, B_2 are to be found.

A schedule is given below for the formation of D_0, D_1, D_2, and a table of the solutions of these equations according to the number of months available.

Next, suppose $C^{(\tau)}$ denotes a function which is subject to annual inequality, and that

$$C^{(\tau)} = \mathrm{A}_0 + \mathrm{A}_1\cos 30^\circ\tau + \mathrm{B}_1\sin 30^\circ\tau.$$

Then
$$C^{(0)} = \mathrm{A}_0 + \mathrm{A}_1,$$
$$C^{(1)} = \mathrm{A}_0 + \tfrac{1}{2}\sqrt{3}\,\mathrm{A}_1 + \tfrac{1}{2}\mathrm{B}_1,$$
$$C^{(2)} = \mathrm{A}_0 + \tfrac{1}{2}\mathrm{A}_1 + \tfrac{1}{2}\sqrt{3}\,\mathrm{B}_1,$$
&c., &c.

In this case the method of least squares gives

$$D_0 = C^{(0)} + C^{(1)} + C^{(2)}\ldots\ldots$$
$$= nA_0 + A_1(1+\tfrac{1}{2}\sqrt{3}+\tfrac{1}{2}\ldots\ldots) + \mathrm{B}_1(0+\tfrac{1}{2}+\tfrac{1}{2}\sqrt{3}\ldots\ldots),$$

$$D_1 = C^{(0)} + \tfrac{1}{2}\sqrt{3}\,C^{(1)} + \tfrac{1}{2}C^{(2)}\ldots\ldots$$
$$= A_0(1+\tfrac{1}{2}\sqrt{3}+\tfrac{1}{2}\ldots\ldots) + A_1(1+\tfrac{3}{4}+\tfrac{1}{4}\ldots\ldots) + \mathrm{B}_1(0+\tfrac{1}{4}\sqrt{3}+\tfrac{1}{4}\sqrt{3}\ldots\ldots),$$

$$D_2 = 0.C^{(0)} + \tfrac{1}{2}C^{(1)} + \tfrac{1}{2}\sqrt{3}\,C^{(1)}\ \ldots$$
$$= \mathrm{A}_0(0+\tfrac{1}{2}+\tfrac{1}{2}\sqrt{3}\ldots\ldots) + \mathrm{A}_1(0+\tfrac{1}{4}\sqrt{3}+\tfrac{1}{4}\sqrt{3}\ldots\ldots) + \mathrm{B}_1(0+\tfrac{1}{4}+\tfrac{3}{4}\ldots\ldots).$$

Tables are given below for the formation of D_0, D_1, D_2, and of the solutions of the equations according to the number of months available.

Reduction of incomplete series of results.

Form for finding D_0, D_1, D_2 where the monthly values are subject to semi-annual inequality.

	i. Monthly values.		ii.	iii. i + ii.	iv. Last 2 of iii revd.	v. iii + iv.	Factor M.	vi. $M \times$ v.	iii − iv.	
0	a	6	a'	$a+a'$	……..	$a+a'$	1	$a+a'$	……………..	
1	b	7	b'	$b+b'$	$f+f'$	$b+\ldots\ldots+f'$	$\frac{1}{2}$	$\frac{1}{2}(b+\ldots\ldots+f')$	$b+\ldots\ldots-f'$	$S_4 = 0{\cdot}866$
2	c	8	c'	$c+c'$	$e+e'$	$c+\ldots\ldots+e'$	$-\frac{1}{2}$	$-\frac{1}{2}(c+\ldots\ldots+e')$	$c+\ldots\ldots-e'$	
3	d	9	d'	$d+d'$	……..	$d+d'$	-1	$-(d+d')$	……………..	
4	e	10	e'	$e+e'$						
5	f	11	f'	$f+f'$		Sum D_0		Sum D_1	Sum $\times S_4$	
									D_2	

Form for finding D_0, D_1, D_2 where the monthly values are subject to annual inequality.

	i. Monthly values.		ii.	iii. i + ii.	iv. i − ii.	v. Last 2 of iv revd.	vi. iv − v.	Factor M.	vii. $M \times$ vi.	viii. iv + v.	Factor M.	M + viii.	
0	a	6	a'	$a+a'$	$a-a'$	……..	$a-a'$	1	$a-a'$	$a-a'$	0	0	
1	b	7	b'	$b+b'$	$b-b'$	$f-f'$	$b-\ldots\ldots+f'$	S_4	$S_4(b-\ldots\ldots+f')$	$b-\ldots\ldots-f'$	$\frac{1}{2}$	$\frac{1}{2}(b-\ldots\ldots-f')$	
2	c	8	c'	$c+c'$	$c-c'$	$e-e'$	$c-\ldots\ldots+e'$	$\frac{1}{2}$	$\frac{1}{2}(c-\ldots\ldots+e')$	$c-\ldots\ldots-e'$	S_4	$S_4(c-\ldots\ldots-e')$	$S_4 = 0{\cdot}866$
3	d	9	d'	$d+d'$	$d-d'$	……..	$d-d'$	0	0	$d-d'$	1	$d-d'$	
4	e	10	e'	$e+e'$	$e-e'$				Sum D_1			Sum D_2	
5	f	11	f'	$f+f'$	$f-f'$								
				Sum D_0									

In both these forms those monthly values which are unknown are to be treated as being zero. For example, if 9 months of observation are available, d', e', f' will be zero.

Rule for finding semi-annual inequality from an incomplete series.

Number of months available.		Coefft. of D_0.	Coefft. of D_1.	Coefft. of D_2.
6	$A_0 =$	$+0·167$		
	$A_2 =$		$+0·333$	
	$B_2 =$			$+0·333$
7	$A_0 =$	$+0·148$	$-0·037$	
	$A_2 =$	$-0·037$	$+0·259$	
	$B_2 =$			$+0·333$
8	$A_0 =$	$+0·136$	$-0·045$	$-0·026$
	$A_2 =$	$-0·045$	$+0·253$	$-0·019$
	$B_2 =$	$-0·026$	$-0·019$	$+0·275$
9	$A_0 =$	$+0·123$	$-0·027$	$-0·047$
	$A_2 =$	$-0·027$	$+0·228$	$+0·011$
	$B_2 =$	$-0·047$	$+0·011$	$+0·241$
10	$A_0 =$	$+0·107$		$-0·041$
	$A_2 =$		$+0·182$	
	$B_2 =$	$-0·041$		$+0·238$

Rule for finding annual inequality from an incomplete series.

Number of months available.		Coefft. of D_0.	Coefft. of D_1.	Coefft. of D_2.
6	$A_0 =$	$+0·977$	$-0·326$	$-1·215$
	$A_1 =$	$-0·326$	$+0·442$	$+0·405$
	$B_1 =$	$-1·215$	$+0·405$	$+1·845$
7	$A_0 =$	$+0·424$		$-0·528$
	$A_1 =$	$+0·250$		
	$B_1 =$	$-0·528$		$+0·990$
8	$A_0 =$	$+0·226$	$+0·062$	$-0·233$
	$A_1 =$	$+0·062$	$+0·230$	$-0·093$
	$B_1 =$	$-0·233$	$-0·093$	$+0·552$
9	$A_0 =$	$+0·146$	$+0·057$	$-0·098$
	$A_1 =$	$+0·057$	$+0·230$	$-0·083$
	$B_1 =$	$-0·098$	$-0·083$	$+0·326$
10	$A_0 =$	$+0·110$	$+0·036$	$-0·036$
	$A_1 =$	$+0·036$	$+0·218$	$-0·048$
	$B_1 =$	$-0·036$	$-0·048$	$+0·218$

We thus get the following rule for the evaluation of A_0, S_{sa}, S_1, S_2, S_4, S_6, K_2, K_1, P from 6, 7, 8, 9, or 10 months of observation:—

Proceed as though the year were complete and find the $\mathfrak{A}$*'s and* $\mathfrak{B}$*'s for as many months as are available. Reduce the* $\mathfrak{A}_2$, $\mathfrak{B}_2$ *by multiplication by* $1/P^{(\tau)}$ *or* $1-0{\cdot}0504\cos(h_0+30°\tau+94°)$.

Analyse $\mathfrak{A}_0^{(\tau)}$, $\mathfrak{A}_1^{(\tau)}$, $\mathfrak{B}_1^{(\tau)}$ *for annual inequality, and* $\mathfrak{A}_2/P^{(\tau)}$, $\mathfrak{B}_2^{(\tau)}/P^{(\tau)}$ *for semi-annual inequality according to the rules for reduction of incomplete series just given.*

Complete the reduction as in § 3.

These rules for reduction do not include the case of 11 months, nor the case where any month in the series is incomplete (*e.g.*, if a fortnight's observation were wanting in one of the months), because these cases may be treated thus:—the $\mathfrak{A}$'s and $\mathfrak{B}$'s return to the same value at the end of a year, and therefore the case of eleven months is the same as that of a missing month at any other part of the year. In both these cases we may interpolate the missing $\mathfrak{A}$'s and $\mathfrak{B}$'s and treat the year as complete.

If three or more weeks of observation were missing they might fall so as to spoil two months, and in this case we should have an incomplete series. It is then to be recommended that the equations of least squares be formed and the equations solved. So many similar cases may arise that it does not seem worth while to solve the equations until the case arises.

§ 5. *Evaluation of* A_0, S_2, S_4, K_2, K_1, P *from a short period of observation.*

If the available tidal observations only extend over a few months, it is useless to attempt the independent evaluation of those tides which we have hitherto found by means of annual and semi-annual inequalities in the monthly harmonic constants. We will suppose that 30 days of observations are available. Then when we neglect the annual tide, and the solar (meteorological) tide S_1, we have from (11) or (17), which give the analysis of 30 days,

$$\mathfrak{A}_0 = A_0,$$

$$\left.\begin{matrix}\mathfrak{A}_1\\ \mathfrak{B}_1\end{matrix}\right\} = \frac{f'H'}{\mathfrak{F}_1}\begin{matrix}\cos\\ \sin\end{matrix}(\kappa'-V'-14°{\cdot}76)+\frac{H_p}{\mathfrak{F}_1}\begin{matrix}\cos\\ \sin\end{matrix}(\kappa_p-V_p+14°{\cdot}76),$$

$$\left.\begin{matrix}\mathfrak{A}_2\\ \mathfrak{B}_2\end{matrix}\right\} = PH_s\begin{matrix}\cos\\ \sin\end{matrix}\kappa_s+\frac{f''H''}{\mathfrak{F}_2}\begin{matrix}\cos\\ \sin\end{matrix}(\kappa''-V''-29°{\cdot}53),$$

$$\left.\begin{matrix}\mathfrak{A}_4\\ \mathfrak{B}_4\end{matrix}\right\} = H_{2s}\begin{matrix}\cos\\ \sin\end{matrix}\kappa_{2s}, \qquad P = 1+0{\cdot}0504\cos(h_0+15°).$$

It is now necessary to assume that the P tide has the same

amount of retardation as the K_1, and that the ratio of their amplitudes is the same as in the equilibrium theory. We also make the like assumption with respect to the K_2 and S_2 tides.

Accordingly we put

$$H_p = \tfrac{1}{3}H', \qquad \kappa_p = \kappa'; \qquad H'' = \tfrac{3}{11}H_s, \qquad \kappa'' = \kappa_s.$$

Now since

$$V' = h_0 - \tfrac{1}{2}\pi - \nu', \qquad V_p = -h_0 + \tfrac{1}{2}\pi, \qquad V'' = 2h_0 - 2\nu'',$$

we have

$$\kappa_p - V_p + 14^\circ\cdot76 = \kappa' - V' - 14^\circ\cdot76 + (2h_0 - \nu' + 29^\circ\cdot53) + \pi,$$

$$\kappa'' - V'' - 29^\circ\cdot53 = \kappa_s - (2h_0 - 2\nu'' + 29^\circ\cdot53).$$

Therefore

$$\left.\begin{matrix}\mathfrak{A}_1\\ \mathfrak{B}_1\end{matrix}\right\} = \frac{f'H'}{\mathfrak{f}_1}\begin{matrix}\cos\\ \sin\end{matrix}(\kappa' - V' - 14^\circ\cdot76)$$
$$- \frac{\frac{1}{3}H'}{\mathfrak{f}_1}\begin{matrix}\cos\\ \sin\end{matrix}(\kappa' - V' - 14^\circ\cdot76 + 2h_0 - \nu' + 29^\circ\cdot53),$$

$$\left.\begin{matrix}\mathfrak{A}_2\\ \mathfrak{B}_2\end{matrix}\right\} = PH_s\begin{matrix}\cos\\ \sin\end{matrix}\kappa_s + \frac{\frac{3}{11}f''H_s}{\mathfrak{f}_2}\begin{matrix}\cos\\ \sin\end{matrix}(\kappa_s - 2h_0 + 2\nu'' - 29^\circ\cdot53).$$

Let us put $\tan\phi = \dfrac{\sin(2h_0 - \nu' + 29^\circ\cdot53)}{3f' - \cos(2h_0 - \nu' + 29^\circ\cdot53)}$,

$$\tan\psi = \frac{f''\sin(2h_0 - 2\nu'' + 29^\circ\cdot53)}{\frac{11}{3}P\mathfrak{f}_2 + f''\cos(2h_0 - 2\nu'' + 29^\circ\cdot53)} \quad \ldots\ldots \quad (19).$$

Then

$$\left.\begin{matrix}\mathfrak{A}_1\\ \mathfrak{B}_1\end{matrix}\right\} = \frac{H'}{\mathfrak{f}'}\frac{[3f' - \cos(2h_0 - \nu' + 29^\circ\cdot53]}{3\cos\phi}\begin{matrix}\cos\\ \sin\end{matrix}(\kappa' - V' - 14^\circ\cdot76 - \phi),$$

$$\left.\begin{matrix}\mathfrak{A}_2\\ \mathfrak{B}_2\end{matrix}\right\} = H_s\frac{\frac{11}{3}P\mathfrak{f}_2 + f''\cos(2h_0 - 2\nu'' + 29^\circ\cdot53)}{\frac{11}{3}\mathfrak{f}_2\cos\psi}\begin{matrix}\cos\\ \sin\end{matrix}(\kappa_s - \psi) \quad \ldots \quad (20).$$

If therefore

$$\left.\begin{matrix}\mathfrak{A}_1\\ \mathfrak{B}_1\end{matrix}\right\} = R_1\begin{matrix}\cos\\ \sin\end{matrix}\zeta_1, \qquad \left.\begin{matrix}\mathfrak{A}_2\\ \mathfrak{B}_2\end{matrix}\right\} = R_2\left.\begin{matrix}\cos\\ \sin\end{matrix}\right\}\zeta_2,$$

we have

$$\kappa' = \zeta_1 + V' + 14^\circ\cdot76 + \phi = \kappa_p,$$

$$H' = \frac{3\mathfrak{f}_1 R_1\cos\phi}{3f' - \cos(2h_0 - \nu' + 29^\circ\cdot53)}, \qquad H_p = \tfrac{1}{3}H'.$$

$$\kappa_s = \zeta_2 + \psi = \kappa'',$$

$$H_s = \frac{\frac{11}{8}\mathfrak{f}_2 R_2 \cos\psi}{\frac{11}{8}P\mathfrak{f}_2 + f''\cos(2h_0 - 2\nu'' + 29^\circ{\cdot}53)},\ H'' = \tfrac{3}{11}H_s \,..\, (21).$$

If there be several months available it is recommended that each 30 days be treated quite independently, so that from each group of days we shall get H', κ' and H_s, κ_s. Then the mean value of $H'\cos\kappa'$ is to be taken as the final value of that function, and $H'\sin\kappa'$ is to be treated similarly; finally H', κ' are to be found. The several values of H_s, κ_s may be treated in the same way. Of course we assume throughout that $\kappa_p = \kappa'$, $H_p = \frac{1}{3}H'$, $\kappa'' = \kappa_s$, $H'' = \frac{3}{11}H_s$, assumptions which are usually nearly correct.

The mean value of $\mathfrak{Z}_0$ must be taken as giving A_0, but at places with a considerable annual tide it is impossible to obtain a good value of mean water mark from a short series of observations.

§ 6. *On the evaluation of the several tides by grouping of mean solar days.*

Let $n(\gamma-\chi)$ denote the speed in degrees per m.s. hour of any one tide, n being equal to 1, 2, 3, 4, 5, or 6. Then $15^\circ/(\gamma-\chi)$ may be called one "special hour." Since $15^\circ/(\gamma-\eta)$ is one m.s. hour, the ratio of the m.s. to the special hour is $(\gamma-\chi)/(\gamma-\eta)$.

Let one m.s. hour be equal to $1-\beta$ special hour, then

$$\beta = 1 - \frac{\gamma-\chi}{\gamma-\eta},\ \text{special hours.}$$

Let it be required to express the 12^h of any m.s. day of a series of days by reference to special time. It is clear that 12^h m.s. time will be specified by one of the 24 special hours, with something less than half a special hour added or subtracted.

Having fixed the 12^h of m.s. time of a particular m.s. day in the special time scale, let us treat that m.s. day as a whole, and consider the incidence of the other 23 m.s. hours in special time. It is clear that in m.s. time we work backwards and forwards from 12^h by subtracting or adding unity, and that in special time we subtract or add $1-\beta$.

If 12^h m.s. time be $x^h+\alpha$, where α lies between $\pm\frac{1}{2}$ special time, the following is a schedule of equivalence :—

Mean solar time.		Special time.
0^h	=	$(x^h-12^h)+(\alpha+12\beta)$
1^h	=	$(x^h-11^h)+(\alpha+11\beta)$
2^h	=	$(x^h-10^h)+(\alpha+10\beta)$
....		
11^h	=	$(x^h-1^h)\ +(\alpha+\beta)$
12^h	=	$x^h\qquad +\alpha$
13^h	=	$(x^h+1^h)\ +(\alpha-\beta)$
....		
22^h	=	$(x^h+10^h)+(\alpha-10\beta)$
23^h	=	$(x^h+11^h)+(\alpha-11\beta)$

In the column of special time it is supposed that 24^h is added or subtracted, so that the result is less than 24^h. For example, if x is 10, the hour column of special time will run 22^h, 23^h, 0^h, ..., 9^h, 10^h, 11^h, ..., 20^h, 21^h.

If the series of days be long x will have all integral values between 0 and 23 with equal frequency, and since α has all values between $+\frac{1}{2}$ and $-\frac{1}{2}$ with equal frequency, the excess of the solar hour above the nearest exact special hour (which may be called the error) will have all its possible values with equal frequency. If the mean solar hours be arranged in a schedule of columns headed 0^h, 1^h, ..., 23^h of special time, each column will be subject to errors which follow the same law of frequency.

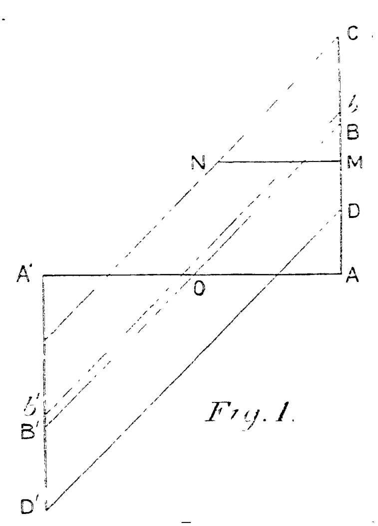


Fig. 1.

Let abscissæ (fig. 1) measured from O along A′OA represent magnitude of α.

Since α lies between $\pm\frac{1}{2}$, the limit of the figure is given by $OA = OA' = \frac{1}{2}$.

If magnitude of error (*i.e.* m.s. — special hour), measured in special time, be represented by ordinates, a line BOB′ at 45° to AOA′ represents all the errors which can arise in the incidence of the m.s. 12^h in the schedule of special time.

If a line bb' be drawn parallel to and above BB′ by a distance β, we have a representation of all the errors of incidence of the m.s. 11^h. If a series of equidistant parallel lines be drawn above and below BB′ until there are 12 above and 11 below, then the errors of all the m.s. hours are represented, the top one showing the errors of the m.s. 0^h and the bottom one the errors of the m.s. 23^h.

Any special hour corresponds with equal frequency with each solar hour, and hence each mode of error occurs with equal frequency.

It is now necessary to consider in how many ways an error of given magnitude can occur. If in the figure AM represents an error of given magnitude, then wherever MN cuts a diagonal line, it shows that an error may arise in one way.

It is thus clear that there are no + errors greater than $\frac{1}{2}+12\beta$, and no — errors greater than $\frac{1}{2}+11\beta$, and

Errors of magnitude.		
$\frac{1}{2}+12\beta$ to $\frac{1}{2}+11\beta$	may arise in	1 way.
$\frac{1}{2}+11\beta$ to $\frac{1}{2}+10\beta$	„	2 ways.
$\frac{1}{2}+10\beta$ to $\frac{1}{2}+9\beta$	„	3 ways.
....		
$\frac{1}{2}-10\beta$ to $\frac{1}{2}-11\beta$	„	23 ways.
$\frac{1}{2}-11\beta$ to $-(\frac{1}{2}-12\beta)$	„	24 ways.
$-(\frac{1}{2}-12\beta)$ to $-(\frac{1}{2}-11\beta)$	„	23 ways.
....		
$-(\frac{1}{2}+9\beta)$ to $-(\frac{1}{2}+10\beta)$	„	2 ways.
$-(\frac{1}{2}+10\beta)$ to $-(\frac{1}{2}+11\beta)$	„	1 way.

The frequency of error is represented graphically in fig. 2. The slope of the two staircases is drawn at 45°, but any other slope would have done equally well.

A frequency curve of this form is not very convenient, and, as there are many steps in the ascending and descending slopes, I substitute the frequency curve shown in fig. 3. This is clearly equivalent to the former one. In fig. 3 all the times shown in fig. 2 are converted to angle at 15° to the hour; ϵ accordingly denotes $15°\beta$.

Fig. 2.

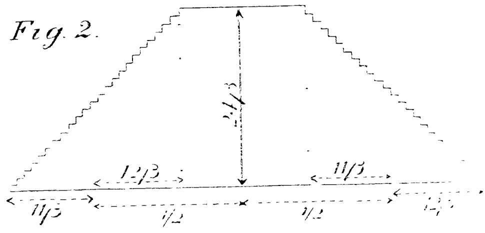
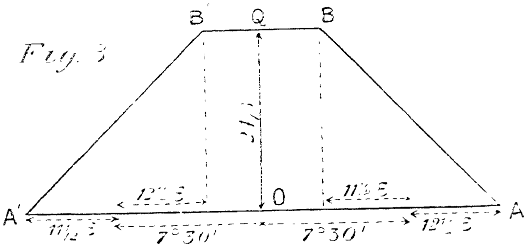


Fig. 3



Now let $\cos n(\theta - x)$ be the observed value of a function whose true value is $\cos n\theta$, and suppose that x, the error of θ, has a frequency $f(x)$; then the mean value of the function deduced from many observations will be

$$\int_{-\infty}^{+\infty} f(x) \cos n(\theta - x)\, dx \div \int_{-\infty}^{+\infty} f(x)\, dx.$$

In our case $f(x)$ is the ordinate of the frequency curve whose abscissa is x.

Let $OQ = h$, $QB = a$, $QB' = b$, $OA = a+h$, $OA' = b+h$; then

$$\int_{-\infty}^{+\infty} f(x)\, dx = (a+b+h)\, h.$$

$$\int_{-\infty}^{+\infty} f(x) \cos n(\theta - x)\, dx$$

$$= \int_0^a h \cos n(\theta - x)\, dx + \int_a^{a+h} (a+h-x) \cos n(\theta - x)\, dx$$

$$+ \int_0^b h \cos n(\theta + x)\, dx + \int_b^{b+h} (b+h-x) \cos n(\theta + x)\, dx,$$

$$= \frac{4}{n^2} \cos n\left[\theta - \tfrac{1}{2}(a-b)\right] \sin \tfrac{1}{2} nh \sin \tfrac{1}{2} n(a+b+h).$$

The algebraical steps involved in the evaluation of these four integrals and subsequent simplification are omitted.

Hence the result is

$$\frac{\sin \frac{1}{2} nh}{\frac{1}{2} nh} \frac{\sin \frac{1}{2} n (a+b+h)}{\frac{1}{2} n (a+b+h)} \cos n \left(\theta - \tfrac{1}{2}(a-b)\right).$$

By reference to the figure it is clear that

$$a+b+h = 15^\circ, \quad h = 24\epsilon, \quad a = 7\tfrac{1}{2}^\circ - 11\tfrac{1}{2}\epsilon, \quad b = 7\tfrac{1}{2}^\circ - 12\tfrac{1}{2}\epsilon, \quad a-b = \epsilon.$$

Write, then,

$$\mathfrak{F}_n = \frac{12\, n\epsilon}{\sin 12\, n\epsilon} \; \frac{\frac{15}{2} n}{\sin \frac{15}{2} n},$$

and we obtain as the mean value of $\cos n\theta$, when found in this way,

$$\frac{1}{\mathfrak{F}_n} \cos n \left(\theta - \tfrac{1}{2}\epsilon\right).$$

It is obvious that if we had begun with $\sin n\theta$, the argument in the result and the factor $\mathfrak{F}_n$ would have been the same. Accordingly, a function $R' \cos (n\theta - \zeta')$ would yield the result $\frac{R'}{\mathfrak{F}_n} \cos \left[n \left(\theta - \frac{1}{2}\epsilon\right) - \zeta' \right]$. If 24 equidistant results of this sort are submitted to harmonic analysis to find A_n, B_n, we shall get—

$$A_n = \frac{R'}{\mathfrak{F}_n} \cos \left(\zeta' + \tfrac{1}{2} n\epsilon\right) = R \cos \zeta, \text{ suppose,}$$

$$B_n = \frac{R'}{\mathfrak{F}_n} \sin \left(\zeta' + \tfrac{1}{2} n\epsilon\right) = R \sin \zeta, \text{ suppose.}$$

Accordingly $\quad R = \dfrac{R'}{\mathfrak{F}_n}, \qquad \zeta = \zeta' + \tfrac{1}{2} n\epsilon.$

But it is required to find R', ζ', so that

$$R' = \mathfrak{F}_n R, \qquad \zeta' = \zeta - \tfrac{1}{2} n\epsilon.$$

Thus when the 24 observed hourly tide heights on any m.s. day are regrouped so that the observed height at 12^h m.s. time is reputed to appertain to an exact special hour, and each of the previous and subsequent hourly values of that m.s. day are reputed to belong to previous and subsequent exact special hours; and when a long series of m.s. days are treated similarly, and when the mean heights of water at each of the 24 special hours are harmonically analysed, we shall obtain the required result by augmenting R by a factor $\mathfrak{F}_n$, and by subtracting $\frac{1}{2} n\epsilon$ from ζ.

The values of $\mathfrak{f}_n$ and of $\frac{1}{2}n\epsilon$ will be different for each kind of tide, and the following table gives their numerical values.

Table of $\mathfrak{f}_n$ and $\frac{1}{2}n\epsilon$.

Initial of tide.	n.	log $\mathfrak{f}_n$.	$\frac{1}{2}n\epsilon$.
M_1	1	0·00212	0°·26
M_2	2	0·00849	0°·53
M_3	3	0·01915	0°·79
M_4	4	0·03416	1°·05
M_6	6	0·07767	1°·57
N	2	0·01361	0°·82
L	2	0·00570	0°·24
ν	2	0·01278	0°·78
O	1	0·00535	0°·57
J	1	0·00225	−0°·28
Q	1	0·01149	0°·90
μ	2	0·02016	1°·09
2SM	2	0·00805	−0°·49
MS	4	0·02342	0°·52
λ	2	0·00595	0°·28
2N	2	0·02136	1°·13
OO	1	0·00481	−0°·53
MK	3	0·01438	0°·50
2MK	3	0·02632	1°·09
MN	4	0·04328	1°·35

As it does not appear worth while to evaluate the tides written below the line, no use will be made of the last six results given in this table.

§ 7. *On the periods over which the means are to be taken in evaluating the tidal constants.*

We have considered in previous sections the treatment of the group of tides which are associated with solar time, when the period of observation is less than a year, and we have now to consider the other tides.

It is important that the means be taken over such a number of days that the perturbation arising from other tides shall be minimised.

The perturbation between semi-diurnal and diurnal tides is always negligible. It is therefore only necessary to consider the action of the tides M_2, S_2 in the case of semi-diurnal tides, and that of K_1 and O for diurnal tides.

It is easy to see that the influence of a disturbing tide is evanescent when the means are taken over a period such that the excess of the argument of the disturbed over that of the disturbing tide has increased through a multiple of 360°. As, however, we are working with integral numbers of days, and as the speeds of tides are incommensurable, this condition cannot be exactly satisfied.

From this consideration it appears that to minimise the perturbation of S_2, 2SM, μ by M_2 (and *vice versâ*) we must stop at an exact multiple of a semi-lunation. To minimise the effect of M_2 on N and L, and of K_1 on J and Q, we must stop at an exact multiple of a lunar anomalistic period. To minimise the effect of M_2 on ν, we must stop at a multiple of the period $2\pi/(\sigma+\varpi-2\eta)$. To minimise the effect of K_1 on O, we must stop at an exact multiple of a semi-lunar period.

For the quater-diurnal tide, MS, it is immaterial where we stop, and so it may as well be taken at a multiple of a semi-lunation.

The following table (p. 374) gives the rules derived from these considerations.

§ 8. *On the tides of long period.*

The annual (Sa) and semi-annual (Ssa) tides are evaluated in the course of the work by which other important tides are found. These are the only two tides of long period which have a practical importance in respect to tidal prediction, but the luni-solar fortnightly (MSf), the lunar fortnightly (Mf), and the lunar monthly (Mm) tides have a theoretical interest.

It will therefore be well to show how they may be found. The process is short, and, although it is less accurate than the laborious plan followed in the Indian reductions, it appears to give fairly good results.

For the sake of simplicity, let us consider the tide MSf. Its period is about 14 days, and therefore a day does not differ very largely from a twelfth part of the period. Accordingly, if about two days in a fortnight are rejected by proper rules, the mean heights of water on the remaining days may be taken as representatives of twelve equidistant values of water height.

I therefore go through the whole year and reject, according to proper rules, the daily sums of the 24 hourly heights corresponding to certain 69 of the days out of 369. The remaining 300 values are written consecutively into a schedule of 12 columns and 25 rows, of which each corresponds to a half lunation. The 12 columns are summed, and the sums are harmonically analysed for the first pair of harmonic components. These components have to be divided by 24 times 25, or by 600, because the daily mean water height is $\frac{1}{24}$th of the daily sum, and there are 25 semi-lunations.

In the same way the semi-lunar period is about $13\frac{1}{2}$ days, and if

Number of the last day to be included in the evaluation of the several tides for observations extending over any period up to a year.

For M_2, μ, 2SM, MS. Stop with one of the following days (semi-lunations).	For O. Stop with one of the following days (semi-lunar periods).	For N, L, J, Q. Stop with one of the following days (anom. periods).	For ν. Stop with one of the following days (periods $2\pi/(\sigma+\varpi-2\eta)$).
14	13	27	31
29	26	54	63
43	40	—	—
58	54	74+ 8	74+20
73	67	+35	+52
—	—	+63	—
74+14	74+ 7	—	148+10
+28	+21	148+16	+42
+43	+34	+44	—
+58	+48	+71	222+ 0
+73	+62	—	+31
—	—	222+25	+63
148+13	148+ 1	+53	—
+28	+15	—	296+21
+43	+29	296+ 6	+53
+58	+42	+34	—
+72	+56	+61	
—	+70	—	
222+13	—		
+28	222+ 9		
+43	+23		
+58	+37		
+72	+50		
—	+64		
296+13	—		
+28	296+ 4		
+43	+17		
+57	+32		
+72	+45		
—	+58		
	+72		
	—		

we erase by proper rules 45 daily sums out of 369, we are left with 324, which may be written consecutively in a schedule of 12 columns and 27 rows, of which each corresponds to a semi-lunar period. The summing and analysis is the same as in the last case, but the final division is by 24 times 27, or by 648.

In this way we evaluate the luni-solar fortnightly and lunar fortnightly inequalities in the height of the water.

The period of the moon is between 27 and 28 days, and if we erase appropriately about one day in eight we are left with sets of 24 values which may be taken as 24 equidistant values of the daily sums. Accordingly we erase 46 daily sums out of 358, and write the 312 which remain consecutively into a schedule of 24 columns and 13 rows, of which each corresponds to a lunar anomalistic period.

The 24 columns are summed and the sums analysed for the first components. Finally, the components are to be divided by 24 times 13, or by 312. In this way the lunar monthly tide is evaluated.

But the result obtained in this way is, as far as concerns the tide MSf, to some, and it may be to a large, extent fictitious. It represents, in fact, a residuum of the principal lunar tide M_2. That this is the case will now be proved.

Suppose that t_0 is an integral number of days since epoch, being the time of noon on a certain day; then the principal lunar tide M_2 on that day may be written $H_m \cos[2(\gamma-\sigma)(t_0+\tau)-\zeta_m]$, where τ is less than 24 hours. Then the daily sum for that day will be

$$H_m \frac{\sin 24(\gamma-\sigma)}{\sin(\gamma-\sigma)} \cos[2(\gamma-\sigma)t_0+23(\gamma-\sigma)-\zeta_m].$$

Now since t_0 is an integral number of days $2(\gamma-\sigma)t_0$ only differs from $-2(\sigma-\eta)t_0$ by an exact multiple of 360°; hence the argument of the cosine may be written $2(\sigma-\eta)t_0 - 23(\gamma-\sigma)+\zeta_m$.

But the true luni-solar fortnightly tide, which we may denote $fH\cos[2(\sigma-\eta)(t_0+\tau)-\zeta]$, varies so slowly in the course of a day that the daily sum is sensibly equal to

$$24 fH \cos[2(\sigma-\eta)t_0+23(\sigma-\eta)-\zeta].$$

It thus appears that the residual effect of M_2 is of exactly the same form as that of MSf. It becomes, therefore, necessary to clear the harmonic components, determined as described above, from the effects of M_2.

In order to determine the values of these clearances, I found the values of $\cos 2(\sigma-\eta)t$ and $\sin 2(\sigma-\eta)t$ for every noon in a year of 369 days. I then erased the values selected for the treatment of MSf and analysed the remaining values. In this way it was easy to find the effect of the known M_2 tide.

Suppose that A_1, B_1 are the first harmonic components determined by the treatment of a series of daily sums, and that δA_1, δB_1 are the corrections to be applied to them to eliminate the effects of M_2, then I find that if A_m, B_m are the two components of M_2 as determined by the previous method (§ 6) of analysis,

$$\delta A_1 = +0{\cdot}0304\, A_m - 0{\cdot}0171\, B_m,$$

$$\delta B_1 = -0{\cdot}0171\, A_m - 0{\cdot}0304\, B_m,$$

$$C = A_1 + \delta A_1, \qquad D = B_1 + \delta B_1.$$

$$C - 0{\cdot}047\, D = 0{\cdot}992\, \mathrm{f} H \cos \zeta,$$

$$D + 0{\cdot}047\, C = 0{\cdot}992\, \mathrm{f} H \sin \zeta.$$

Whence f being known from Baird's manual (being a function of the longitude of moon's node), H and ζ are determinable. We have also

$$\kappa = \zeta + 2\,(s_0 - \xi - h_0 + \nu) + 11^{\circ}{\cdot}7.$$

In the set of computation forms which I have prepared for use on the present plan, it is shown what days are to be erased for each of the three analyses, and how they are to be entered in schedules, summed, and analysed.

§ 9. *On abridgment in the computations.*

It seemed probable that one decimal of a foot would suffice to express the hourly tide heights. In order to test this, I have taken several individual days of observation at Port Blair, and have found, by harmonic analysis, the time and amplitude of the diurnal and semi-diurnal H.W., first, when the hourly heights are expressed to two decimal places of a foot, and secondly, when they are only entered to the nearest tenth of a foot. I find that the times of H.W. agree within less than a minute of time, and that the amplitudes agree within a fraction of an inch. If this much be true of individual days, the difference of results arising from two or one place of decimals will clearly entirely disappear when a series of days is considered. Hence, by taking as unit the tenth of a foot, or the inch, or even two inches at places with large tides, we may always express all, or nearly all, the heights on which we are to operate by two significant figures. The adoption of this rule not only saves the writing of a large number of figures, but also enormously diminishes the labour of the additions which have to be made.

It also seemed probable that substantial accuracy might be attained from the harmonic analysis of only 12 hourly values instead of 24. In order to test this I took the tidal reductions for Port Blair, Andaman Islands (kindly lent me by the Survey of India), and have compared the results which would have been derived from 12 values with those actually obtained from 24 values by the computers of the Indian Survey. The following tables give the results:—

Semi-diurnal tides.

Initial.	Results from 12 two-hourly values.	Results from 24 hourly values.	Error, (12) – (24).
	ft.	ft.	ft.
S $\begin{cases} A_2 = \\ B_2 = \end{cases}$	+0·6883 +0·6775	+0·6890 +0·6768	–0·0007 +0·0008
M $\begin{cases} A_2 = \\ B_2 = \end{cases}$	–1·7032 +1·0883	–1·7005 +1·0872	–0·0027 +0·0011
K $\begin{cases} A_2 = \\ B_2 = \end{cases}$	–0·1437 –0·2527	–0·1407 –0·2515	–0·0030 –0·0012
L $\begin{cases} A_2 = \\ B_2 = \end{cases}$	+0·0357 +0·0612	+0·0347 +0·0610	+0·0010 +0·0002
N $\begin{cases} A_2 = \\ B_2 = \end{cases}$	+0·3422 +0·2192	+0·3486 +0·2124	–0·0064 +0·0068
ν $\begin{cases} A_2 = \\ B_2 = \end{cases}$	–0·1217 +0·0867	–0·1165 +0·0887	–0·0052 –0·0020
μ $\begin{cases} A_2 = \\ B_2 = \end{cases}$	+0·0857 +0·0383	+0·0849 –0·0388	–0·0008 –0·0005
2SM $\begin{cases} A_2 = \\ B_2 = \end{cases}$	+0·0055 –0·0198	+0·0037 –0·0200	+0·0018 +0·0002

Diurnal tides.

Initial.	Result from 12 two-hourly values.	Result from 24 hourly values.	Error, (12) – (24).
	ft.	ft.	ft.
S $\begin{cases} A_1 = \\ B_1 = \end{cases}$	+0·0175 +0·0223	+0·0185 +0·0216	–0·0010 +0·0007
M $\begin{cases} A_1 = \\ B_1 = \end{cases}$	+0·0120 –0·0168	+0·0059 –0·0173	+0·0061 +0·0005
K $\begin{cases} A_1 = \\ B_1 = \end{cases}$	+0·3815 +0·1398	+0·3847 +0·1396	–0·0032 +0·0002
O $\begin{cases} A_1 = \\ B_1 = \end{cases}$	–0·0818 +0·1335	–0·0729 +0·1386	–0·0089 –0·0051
P $\begin{cases} A_1 = \\ B_1 = \end{cases}$	–0·0167 –0·1287	–0·0178 –0·1280	+0·0011 –0·0007
J $\begin{cases} A_1 = \\ B_1 = \end{cases}$	–0·0193 +0·0315	–0·0167 +0·0347	–0·0026 –0·0032
Q $\begin{cases} A_1 = \\ B_1 = \end{cases}$	+0·0140 –0·0170	+0·0136 –0·0194	+0·0004 +0·0024

The mean discrepancy in the case of the semi-diurnal tides is 0·0022 ft., and the greatest is +0·0068; in the case of the diurnal tides the mean discrepancy is 0·0026 ft., and the greatest is 0·0089.

In tidal work results derived from different years of observation differ far more than do these two sets of results, and hence the analysis of 12 two-hourly values for diurnal and semi-diurnal tides gives adequate results.

I find that this abbreviation does not give satisfactory results for quater-diurnal tides, and the sixth harmonic is not derivable from 12 values. Therefore, when these tides are to be evaluated the 24 hourly values must be used.

It will still be necessary to write all the 24 hourly heights on each computing strip, but when the strips are put into any one of the arrangements, except where quater-diurnal tides are required, we need only add up the columns 0, 2, 4, ..., 22, and may omit the columns 1, 3, ..., 23.

§ 10. *On a trial of the proposed method of reduction.*

As already mentioned, I have the tidal reductions for one year (beginning April 19, 1880) for Port Blair, Andaman Islands. I am thus able to make a comparison between the results of the old method and of the new. The computation was, in large part, done for me by Mr. Wright.

It appeared sufficient to evaluate the tides of the S series and those allied with them, the tides of the M series, and the tide Q; also the tides of long period MSf, Mf, Mm.

The S series test the new process of harmonic analysis of monthly harmonic components for annual and semi-annual inequalities. I chose M because it is the most important tide, and Q because it puts the proposed method of grouping to a severe test, and is very small in amplitude.

In the Q time scale the day is $26^h\ 52^m$ of mean solar time, from which it follows that one of the 24 mean solar hourly observations may fall as much as $2^h\ 0^m$ away from the exact Q hour to which it is reputed to belong. Thus the hourly observations are arranged in wide groups round the Q hours, and the hypothesis involved in the method is put to a severe strain.

Lastly, the results for tides of long period test my proposed abridgment.

It will be seen in the table on p. 379 that the two methods give results in close agreement. There is, however, a sensible discrepancy in the K_2 tide, but in this case I am inclined to accept the new value as better than the old one. This tide is governed by sidereal time, which differs but little from mean solar time. Hence, in the Indian

Port Blair. 1880–81.

	I. Indian calculation.	N. New method.	I−N (height).	I−N (phase).
	ft.	ft.	ft.	
A_0 =	4·792	4·795	−0·003	
Sa { H =	·299	·299	000	
Sa { κ =	163°	162°	..	+1°
Ssa { H =	·106	·111	−·005	
Ssa { κ =	165°	164°	..	+1°
T { H =	·099*	·094		
T { κ =	313°*	339°		
R { H =	·020*	·004		
R { κ =	326°*	312°		
S_1 { H =	·028	·026	+·002	
S_1 { κ =	49°	53°	..	−4°
S_2 { H =	·966	·973	−·007	
S_2 { κ =	316°	315°	..	+1°
S_4 { H =	·003	·003	000	
S_4 { κ =	107°	105°	..	+2°
K_1 { H =	·403	·401	+·002	
K_1 { κ =	326°	326°	..	0°
K_2 { H =	·286	·268	+·018	
K_2 { κ =	314°	311°	..	+3°
P { H =	·130	·139	−·009	
P { κ =	324°	323°	..	+1°
M_1 { H =	·014	·013	+·001	
M_1 { κ =	23°	34°	..	−11°
M_2 { H =	2·042	2·043	−·001	
M_2 { κ =	279°	279°	..	0
M_3 { H =	·004	·004	000	
M_3 { κ =	20°	54°	..	−34°
M_4 { H =	·003	·006	−·003	
M_4 { κ =	167°	264°	..	−97°
M_6 { H =	·004	·005	−·001	
M_6 { κ =	342°	315°	..	+27°
(24 values) Q { H =	·023	·023	000	
(24 values) Q { κ =	236°	233°	..	+ 3°
(12 values) Q { H =	·023	·022	+·001	
(12 values) Q { κ =	236°	234°	..	+ 2°

* These are derived from 1880–82.

Tides of Long Period.

	I. Indian calculation.	N. New method.	I−N (height).	I−N (phase).
	ft.	ft.	ft.	
MSf $\begin{cases} H = \\ \kappa = \end{cases}$	·045 163°	·019 168°	+ ·026 ..	−5°
Mf $\begin{cases} H = \\ \kappa = \end{cases}$	·056 356°	·056 356°	000 ..	0
Mm $\begin{cases} H = \\ \kappa = \end{cases}$	·016 12°	·020 13°	− ·004 ..	−1°

method of grouping, considerable errors of incidence of the S hours in the K time scale prevail for many days together, and the method seems of doubtful propriety. The same is true of the P tide, and here also the two methods give somewhat different results.

The accuracy with which the very small Q tide comes out, whether from 24 values or only from 12, is surprising, and may perhaps be, to some extent, due to accident. It shows, however, that the present method may be safely applied, even when the special time scale differs considerably from mean solar time.

The results for the tides of long period are quite as close to the old values as could be expected.

§ 11. *A comparison of the work involved in the new and old methods of reduction.*

It has been usual in the Indian reductions to use three digits in expressing the height of water, and there have been 15 series, or even more. Now $3 \times 24 \times 365 \times 15$ is 394000; hence the computer has had to write that number of figures in reducing a year of observation. This does not include the evaluation of the annual and semi-annual tides, so that we may say that there have been about 400,000 figures to write.

I propose to express the heights by two digits, and they only have to be written once. Thus, in the present plan, the number of figures to write is $2 \times 24 \times 365$, or 17,500. Thus the writing of 382,000 figures is saved.

In the old method the computer had to add together all the digits written, say, 394,000 additions of digit to digit.

I propose to use 24 hourly values in three series, viz., S, M, and MS, and 12 two-hourly values in eight others. Therefore, the number of additions will be $3 \times 2 \times 24 \times 365 + 8 \times 2 \times 12 \times 365$ or 123,000. Thus 270,000 additions are saved.

We may say that formerly there were about 800,000 operations (writing and addition), and that in the present method there will be about 140,000. This estimate does not include a saving of several thousands of operations in obtaining the tides of long period. I am therefore within the mark when I claim that the work formerly bestowed on one year of observation will now reduce at least five years.

It has been found that the manufacture of my computing strips of xylonite is rather expensive, but as it formerly cost in England rather more than £20 to reduce a year of observation, the cost of the apparatus will be covered by the saving in the reduction of a single year, and it will serve for any length of time.

§ 12. *On the completion of the record for short gaps and long gaps.*

In any long series of tidal observations there are usually some breaks in the record in consequence of the stoppage of the clock of the tide gauge, or from some other cause. Now the process of elimination by grouping depends essentially on the completeness of the record, and it is therefore necessary to fill in blanks by interpolation.

Such interpolation has not been usual in the operations of the Indian Survey, and it might be thought that the complete omission of the missing entries is the proper course to take; but it is easily shown that this treatment is exactly equivalent to the assumption that the water remained stagnant at mean sea level during the whole time of stoppage of the gauge. It is obvious, therefore, that any conjectural values are better than none.

The process by which it is proposed to interpolate is best shown by an example.

At Port Blair (beginning April 19, 1880) the column of 6^h from days 99 to 112 gives the heights shown in the first column of the table below. I suppose that the tide gauge broke down on day 103, and only came into action again on day 110.* There was really no break down, and the actuality during the supposed hiatus is shown in the last column but one.

Now if we look back about a month we find that the water stood about the same height at the same hour of the day (viz., 6^h). Then the "previous record" (which is complete) beginning at 69^d is entered in the next column. Similarly a "subsequent record" is found about a month later, and is entered in a third column. The mean of the previous and subsequent records is then taken as giving the values to be interpolated.

* The days are here numbered from 1, instead of from 0. This has been the usage in India hitherto.

The last two columns contain a comparison between the interpolation and what in the present case we know to have been actuality. There is a mean error of 0·20 ft. Thus it is clear that a fair record may be interpolated even with so long a break as a week.

In this example I have only shown the interpolation for one column, but of course all the other twenty-three columns would really have to be treated similarly.

I find by trial that the result would be a little improved by a graphical method, but that process is slightly more troublesome than the numerical one.

Table of Interpolation.

Defective record.		Previous record.		Subsequent record.		Mean of previous and subsequent.	Actuality.	Error.
Day.	6^h.	Day.	6^h.	Day.	6^h.			
99	2·54	69	2·02	129	2·56	2·29		
100	3·13	70	2·83	130	3·07	2·95		
101	3·86	71	3·70	131	3·70	3·70		
102	4·40	72	4·55	132	4·27	4·41		
103	..	73	5·10	133	4·88	4·99 Interpolation.	4·83	+0·16
104	..	74	5·60	134	5·27	5·44 Interpolation.	5·24	+0·20
105	..	75	5·72	135	5·35	5·54 Interpolation.	5·39	+0·15
106	..	76	5·67	136	5·28	5·48 Interpolation.	5·18	+0·30
107	..	77	5·59	137	4·92	5·26 Interpolation.	4·90	+0·36
108	..	78	5·04	138	4·44	4·74 Interpolation.	4·53	+0·21
109	..	79	4·62	139	3·57	4·10 Interpolation.	4·05	+0·05
110	3·32	80	3·81	140	2·95	3·38		
111	2·64	81	3·26	141	2·50	2·38		
112	2·17	82	2·73	142	1·99	2·36		

It may happen that the hiatus is too long for treatment in this way. I do not think it would be safe to treat much more than a fortnight by interpolation.

It has been shown in § 4 how the tides associated with S are to be treated where the record is deficient, and it remains to consider the other tides.

In § 7 are given the days with which we must stop in the analysis of an incomplete year, and this table affords us the means of treating a long hiatus in the observation.

We may in fact omit all the entries between any two of the numbers given in the table without seriously affecting the result.

Let us suppose, as an example, that the tide gauge broke down on day 210 and was only repaired and in operation again on day 226. Now 210 is 148+62, and 225 is 222+3.

Then we see by the table in § 7 that in finding the means for

M, 2SM, MS, when the computing strips are written for the third time, we must remove strips 59, 60, 61 (which have numbers written on them) and may leave the remaining strips of that writing which are blank. When the strips are written for the fourth time strips 0, 1, 2, 3 will be blank, but we must remove strips 4 to 13 inclusive. When all the strips are used in a complete year there are 369, and this is the divisor used in obtaining the harmonic constants, but when there is this supposed hiatus we do not use 15 strips of the third writing and 14 strips of the fourth writing, so that the divisor will be 340.

Again, when we are evaluating O in the third writing, strips 57, 58, 59, 60, 61 must be removed, and in the fourth writing strips 4 to 9 inclusive. In a complete year the divisor is 369, but we now do not use 17 strips of the third writing and 10 of the fourth writing, so that the divisor becomes 342.

Again, in evaluating N, L, J, Q, in the third writing we remove strips 45 to 61 inclusive, and in the fourth writing strips 4 to 25 inclusive. The divisor is reduced from 358 to 303.

Lastly in evaluating ν, in the third writing strips 43 to 61 inclusive are removed, and in the fourth writing strips 4 to 31 inclusive. The divisor is reduced from 350 to 287.

Any hiatus, be it long or short, may be treated in this way, but it is clear that if it be short enough to treat by interpolation, it is best to adopt that method.

INSTRUCTIONS FOR USING THE COMPUTING APPARATUS.

The apparatus for the reduction of tidal observations, together with computation forms, can be purchased from the Cambridge Scientific Instrument Company at a price (as far as can be now foreseen) of about £8.

In case of any insufficiency in the following instructions recourse must be taken to the preceding paper.

On the degree of accuracy requisite in the hourly heights.

It will usually be sufficient if the heights be measured to within one-tenth of a foot, and the decimal point may, of course, be omitted in computation.

This gives amply sufficient accuracy at a place where the semi-range of the principal lunar tide is 2 ft., and where spring range is from 6 ft. to 7 ft.

At some places with small tides a smaller unit might be necessary, and at others with very large tides a unit of 2 in., or of a fifth of a foot, might suffice.

Whatever unit of length be taken it is important, for the saving of work, and it is sufficient, that all or nearly all the heights should be expressed by two digits.

Completion of record.

If there is an accidental break in the record, it is very important that it should be completed according to the method shown in § 12, or by some other equivalent plan.

The computation forms are drawn up on the supposition that the year of observation is complete, but with proper alterations, which will now be indicated, they may be used in other cases.

In § 4 it is shown how to treat the tides of the S group when the observations have been subject to a long stoppage in the course of the year, and also when the observations extend over any period from six months to a year.

In § 5 it is shown how to treat the S group for a short period of observation.

If the stoppage be a long one, the method explained in § 12 must be adopted for all the other tides. The same section also shows the treatment for observations extending over any period, long or short, less than a year.

Entries and summations.

The computing strips are intended to take writing in *pencil* or *liquid Indian ink*, but not in common ink.

They are to be cleaned with a damp cloth, and a little soda may be put in the water if they become greasy.

Lay the red S sheet on one drawing board and set up the strips with their ends abutting against the corresponding numbers. The strip numbered 60 is also to be put on the board.

Write the hourly heights for each day on the strip bearing the corresponding number, strip 0 for day 0, strip 1 for day 1, and so on up to strip 73 for day 73. The 24 hourly heights are to be written in the 24 divisions of each strip, beginning on the left with 0^h and ending on the right with 23^h.

Remove strip 60.

Sum the 24 columns formed by the divisional marks on consecutive sets of 30 strips. Thus, days 0 to 29 afford 24 sums; days 30 to 59 afford the second set of 24 sums; days 61 to 73 afford 24 sums, which are the beginning of a third 30, to be completed when the second set of 74 days shall have been written on the strips.

The numbers 0, 1, 2, ..., 23, 0, 1, ..., 23 at the head and foot of the guide sheet indicate the hours corresponding to the columns.

The sums of the columns on the board are to be entered in the

corresponding columns of the form "Hourly sums of S series in twelve months."

Lay the red M guide sheet on the other drawing board, and transfer the strips from the first board to the new arrangement shown by the zigzag lines, strip 60 being now reintroduced.

There will now be 48 columns (more exactly 47, since one of the columns will be found to have nothing in it), numbered at top and bottom 0, 1, ..., 23, 0, 1, ..., 23. Each of the 48 columns is to be summed from bottom to top (not as for S in groups of 30), and the sums are to be entered in the form "Sums of series M." The 24 sums which come from the left half of the board will be entered in the row marked "red left," those from the right in the row marked "red right."

Lay the red N sheet on the other board, and transfer the strips.

In accordance with § 9, it will now usually suffice to sum only the columns appertaining to the even hours 0, 2, 4, ..., 22; as these hours are repeated twice, there will now be 24 columns to sum.

The sums are then to be entered on the form "Sums of series N," in the alternate columns. The complete form is provided, so that all the 24 hourly values may be used if it be thought desirable, but this labour seems unnecessary, at least in a long series of observations.

Lay the successive red sheets on the vacant board, transfer, sum, and enter, until all the red sheets are exhausted.

In the case of S, M, MS the sums of all the columns are necessary, but in the other eight arrangements only the sums of the alternate columns, those of the even hours, are usually necessary. For a short series of observations it may be best to use all the columns, but in this case it will certainly not be worth while to attempt the evaluation of ν, J, Q, μ, 2SM, which are all small in amount.

If the tides of long period MSf, Mf, Mm are required, the 24 numbers written on each strip must be added together, and the sum entered in the form "Long period tides—daily sums."

Clean the strips.

In exactly the same way work through the next 74 days, from 74^d to 74^d+73^d, with yellow guide sheets. Then clean the strips, and take another 74 days with green guide sheets, and so on with the blue and violet.

In the last (violet) set attention must be paid to the rules as to the places where the analysis is to stop in each arrangement.

If the year of observation is so incomplete that the hiatus cannot be made good by interpolation, or if the series does not run over the complete year, the series must stop with one of the days specified in the table at the end of § 7, and a note must be made of the number of days used in each series.

The strips marked for omission on the violet sheets, or those

selected for omission under the rules of § 7, may be hidden by a sheet of paper when the summations are being made.

The additions of S, M, MS may be verified by proving that the grand total of all the numbers (inclusive of omitted strips in S) written in each of the sets of 74 days is the same in whatever way they are arranged. Thus, the sum of the 48 columns should be equal to the sum of the daily sums. An incomplete verification in the other arrangements, when only half the columns are summed, is found by showing that the sum of all the hourly sums of each 74 days is nearly equal to half the grand total of all the numbers written in that period of 74 days.

When the guide sheets become worn with many pin pricks they may easily be patched with adhesive paper. There seems no reason why this patching should not go on almost indefinitely.*

* It is possible that it may be desired to evaluate the tides OO and 2N, for which no guide sheets are provided. I therefore, give instructions for the preparation of guide sheets for these cases. They will be understood by any one who has the set of guide sheets before him. With the instructions given below, the computer might indeed set up the strips without a guide-sheet.

I describe the staircase as descending from left to right or from right to left, and I define a short step as being one space down and one space to the left or right, as the case may be, and a long step as one space down and two to the left or right, as the case may be. When I say, for example, that a short follows 2, I mean that 2 to 3 is a short step. The first mark on each sheet is specified by its incidence in the row of hours at the top.

OO; descending from left to right.

The sequence is long several times repeated and then short.

Red; 0 between 0^h and 1^h; shorts follow 2, 7, 13, 19, 24, 30, 36, 41, 47, 53, 58, 64, 69.

Yellow; 0 between 15^h and 16^h; shorts follow 1, 7, 12, 18, 24, 29, 35, 41, 46, 52, 57, 63, 69.

Green; 0 between 6^h and 7^h; shorts follow 0, 6, 12, 17, 23, 29, 34, 40, 45, 51, 57, 62, 68.

Blue; 0 between 21^h and 22^h; shorts follow 0, 5, 11, 17, 22, 28, 33, 39, 45, 50, 56, 62, 67.

Violet; 0 between 11^h and 12^h; shorts follow 5, 10, 16, 21, 27, 33, 38, 44, 50, 55, 61, 67, 72.

The last strip used for a year is 72.

2N: descending from right to left.

The sequence is long, long, short, long, long, short, and at intervals three longs and a short.

Red; 0 between 22^h and 23^h; shorts follow 1, 4, ..., 16; 20, 23, ..., 35; 39, 42, ..., 57; 61, 64, ..., 70.

Yellow; 0 between 18^h and 19^h; shorts follow 2; 6, 9, ..., 21; 25, 28, ..., 40; 44, 47, ..., 59; 63, 66, ..., 72.

Green; 0 between 13^h and 14^h; shorts follow 1, 4; 8, 11, ..., 26; 30, 33, ..., 45; 49, 52, ..., 64; 68, 71.

Blue; 0 between 8^h and 9^h; shorts follow 0, 3, 6, 9; 13, 16, ..., 28; 32, 35, ..., 50; 54, 57, ..., 69; 73.

Hourly sums and harmonic analysis.

Complete the summations in the forms for hourly sums, and copy into the forms for harmonic analysis. In this copying it will generally suffice if the last figure in the hourly sums be omitted; for example, if the observations are entered to the nearest tenth of a foot the hourly sums will be given in the same unit, and it will suffice if the hourly sums analysed be written to the nearest foot.

There are 12 analyses (one for each month of 30 days) for the hourly sums in S, and one analysis for each of the other 10 arrangements. All the forms are provided with spaces for 24 hourly sums, but in the eight series N, L, ν, O, Q, J, μ, 2SM, where only 12 values will commonly be used, the entries will only be made on the alternate rows of 0^h, 2^h, ..., 22^h. In these cases the divisor 12, which occurs in the penultimate stage of finding the A's and B's, must be replaced by 6.

The large divisors (viz., 369 for M, μ, 2SM, MS; 369 for O; 358 for N, L, J, Q; and 350 for ν) represent the number of days under reduction, and must be altered appropriately (see table, § 7) if there be long gaps in the observations, or if the year be incomplete, or if the series be a short one.

If some one of the monthly analyses of S is deficient the missing $\mathfrak{A}$'s and $\mathfrak{B}$'s are to be made good by interpolation.*

It is then necessary to analyse the monthly values of the $\mathfrak{A}$'s and $\mathfrak{B}$'s derived from the 12 analyses of S. We thus obtain A_0, A_1, B_1, A_2, B_2, C_0, c_0, C_1, D_1, c_1, d_1, E_0, e_0, E_1, F_2, e_1, f_1, E_2, F_2, e_2, f_2,. The rules for these analyses when the year is incomplete are given in § 4, and the computation forms only apply to the case of the complete year.

Astronomical data and final reduction.

Determine from the 'Nautical Almanac' and Major Baird's 'Manual of Tidal Observations'* the astronomical data at 0^h local M.T. on day 0, and proceed according to the form to find the initial arguments and factors for reduction. The astronomical data are then to be used in the forms for final reduction.

We have generally $B = R \sin \zeta$, $A = R \cos \zeta$; the forms are arranged so that colog A is to be added to log B to find log tan ζ, and thence ζ. If ζ lies between $-45°$ and $45°$ or between $135°$ and $225°$, log sec ζ is added to log A to find R; if ζ lies between $45°$ and $135°$ or between $225°$ and $315°$, log cosec ζ is added to log B to find R.

Violet; 0 between 4^h and 5^h; shorts follow 2, 5, ..., 14; 18, 21, ..., 33; 37, 40, ..., 52; 56, 59, ..., 71.

The last strip used for a year is 61.

* Taylor and Francis, London, 1886, price *7s. 6d.*

Accordingly the computation form has log sec ζ, room being left for the syllable "co" if necessary; underneath this is written log and the computer will insert A or B as the case may be.

There is usually required also a numerical factor 1/f or $\mathfrak{f}$, or both; the logarithms of the 1/f are found amongst the astronomical data, and the logarithms of the constant $\mathfrak{f}$'s are printed in the forms in their proper places.

The treatment of the 21 harmonic components derived from the harmonic analysis of the 5 $\mathfrak{A}$'s and $\mathfrak{B}$'s is shown in the forms.

The tides of long period.

The processes involved in the evaluation of these tides are sufficiently shown in the forms.

Postscript. December 17, 1892.

Correction to previous paper.

An error has been detected on p. 333 of my paper "On the Harmonic Analysis of Tidal Observations of High and Low Water," 'Roy. Soc. Proc.,' vol. 48 (1890).

In the example of reduction certain tabular values, extracted from Baird's 'Manual,' have the wrong signs attributed to them. Since in 1887 the longitude of the moon's node lay between 0° and 180°, the signs of ν, ξ, ν', $2\nu''$ should be positive, instead of negative as stated on p. 333. When this error is corrected the reduction leads to the following comparison:—

			Computed. ft.	Mean of 9 yrs. obs. ft.
M_2	H	=	3·98	4·04
	κ	=	330°	330°
S_2	H	=	1·68	1·63
	κ	=	2°	3°
K_2	H	=	0·46	0·41
	κ	=	2°	352°
N	H	=	1·04	1·00
	κ	=	317°	313°
L	H	=	0·11	0·09
	κ	=	237°	308°
K_1	H	=	1·24	1·40
	κ	=	41°	45°
O	H	=	0·69	0·66
	κ	=	59°	48°
P	H	=	0·41	0·40
	κ	=	41°	43°

If the calculations had been conducted by rigorous methods, the two columns would have agreed nearly with one another.

I may mention that I have copies of a table of $(\gamma-\sigma)\,t$ up to 90 days (see p. 304 of the paper here referred to) which I shall be glad to give to any one actually engaged in the reduction of H. and L.W. observations.

II. "On some new Reptiles from the Elgin Sandstone." By E. T. NEWTON, F.G.S. Communicated by Sir ARCHIBALD GEIKIE, F.R.S. Received November 28, 1892.

(Abstract.)

During the last few years a number of Reptilian remains have been obtained from the Elgin Sandstone at Cuttie's Hillock, near Elgin, which are now in the possession of the Elgin Museum and of the Geological Survey. These specimens represent at least eight distinct skeletons, seven of which undoubtedly belong to the Dicynodontia, and one is a singular horned Reptile, new to science. All the remains yet found in this quarry are in the condition of hollow moulds, the bones themselves having entirely disappeared. In order, therefore, to render the specimens available for study, it was necessary, in the first place, so to display and preserve these cavities that casts might be taken which would reproduce the form of the original bones. Gutta-percha was found to be the most suitable material for taking these impressions; and in some instances, especially in the case of the skulls, the casts had to be made in several parts and afterwards joined together.

The first specimen described is named *Gordonia Traquairi;* it is the one noticed by Dr. Traquair in 1885, and referred to the Dicynodontia; besides the skull, it includes fragmentary portions of other parts of the skeleton, and is contained in a block of sandstone which has been split open so as to divide the skull almost vertically and longitudinally. The two halves have been so developed that casts made from them exhibit the left side and upper surface, as well as the main parts of the palate and lower jaw. In general appearance this skull resembles those of *Dicynodon* and *Oudenodon.* The nasal openings are double and directed laterally; the orbits are large and look somewhat forwards and upwards. The supra-temporal fossa is large, and bounded above by the prominent parieto-squamosal crest, and below by the wide supra-temporal bar, which extends downwards posteriorly to form the long pedicle for the articulation of the lower jaw. There is no lower temporal bar. The maxilla is directed downwards and forwards to end in a small tusk. Seen from above, the skull is narrow in the inter-orbital and nasal regions, but wide posteriorly across the temporal bars, although the brain-case itself is very

narrow. There is a large pineal fossa in the middle of a spindle-shaped area, which area is formed by a pair of parietals posteriorly and a single intercalary bone anteriorly.

The palate is continuous with the base of the skull; the pterygoids on each side send off a distinct process to the quadrate region. Towards the front the median part of the united pterygoids arches upwards, and the outer sides descend, forming a deep groove; from the evidence of other specimens it is clear that the palatines, extending inwards, converted this groove into a tube, and thus formed the posterior nares. The ramus of the lower jaw is deep, with a large lateral vacuity, and the two rami are completely united at the symphysis. The back of this skull is not seen, but two other specimens, referable to this same genus, show that the occiput had two post-temporal fossæ on each side.

This specimen is distinguished from *Dicynodon* by the presence of two post-temporal fossæ on each side of the occiput, by the small size of the maxillary tusk; and probably by the elongated spindle-shaped area enclosing the pineal fossa, and also by the slight ossification of the vertebral centra.

A second and much smaller specimen, provisionally referred to *G. Traquairi*, has, besides the skull, a fore-limb well preserved. The humerus of this shows the usual Anomodont expansion of its extremities; its large deltoid crest is angular, and set obliquely to the distal end.

Three other species are referred to the same genus, namely:—

Gordonia Huxleyana, which is distinguished from *G. Traquairi* by its proportionately wider and more depressed skull, and by the absence of the concavity between the orbits which is present in the latter species. The humerus has the distal extremity oblique to the deltoid crest, which was probably rounded and not angular.

G. Duffiana has the skull even wider than in *G. Huxleyana*, and the portion of a humerus found with this skeleton has the two extremities set nearly at right angles to each other.

G. Juddiana has an elongated skull resembling that of *G. Traquairi*, but the parietal crests are less developed, the bones of the nasal region are much thickened and overlap the nasal apertures, the small tusk is placed a little further back and points more directly downwards, and the pineal fossa is smaller than in either of the other species.

A second generic form is named *Geikia Elginensis*. This is a skull nearly allied to *Ptychognathus*, Owen, but is distinguished by its shorter muzzle and the entire absence of teeth; the upper part of the skull, between the orbits, is also peculiar, forming a deep valley open anteriorly, with a ridge on each side, the anterior end of which forms a large prominence above and in front of the orbit. The occiput has

only one (the lower) post-temporal fossa open on each side. The maxilla is produced into a tooth-like prominence, which occupies a similar position to the tusks of *Gordonia;* but the bone is too thin to have supported a tooth, and in all probability it was covered by a horny beak. The lower jaw has a strong symphysis, a distinct lateral vacuity, and the oral margin, at the front of each ramus, bears a rugose prominence.

Elginia mirabilis is the name proposed for the skull of a Reptile, which, on account of the extreme development of horns and spines, reminds one of the living Lizards *Moloch* and *Phrynosoma.* The exterior of this skull is covered in by bony plates, the only apertures being the pair of nostrils, the orbits, and the pineal fossa. The surfaces of the bones are deeply pitted, as in Crocodiles and Labyrinthodonts. The horns and spines, which vary from $\frac{1}{4}$ in. to nearly 3 in. in length, are found upon nearly every bone of the exterior. The development of the epiotics and the arrangement of the external bones resemble more the Labyrinthodont than the Reptilian type of structure, while the palate, on the other hand, conforms more nearly to the Lacertilian type, and, with the exception that the pterygoids are united in front of the pterygoid vacuity, agrees with the palate of *Iguana* and *Sphenodon.* There are four longitudinal ridges along the palate, some of which seem to have carried teeth. The oral margin was armed with a pleurodont dentition, there being on each side about twelve teeth with spatulate crowns, laterally compressed and serrated. With the exception of the smaller number of the teeth, we have here, on a large scale, a repetition of the dentition of *Iguana.* This peculiar skull seems to show affinities with both Labyrinthodonts and Lacertilians, and is unlike any living or fossil form; its nearest, though distant, ally apparently being the *Pareiasaurus* from the Karoo Beds of South Africa.

III. "The Electromotive Properties of the Skin of the Common Eel." By E. WAYMOUTH REID, Professor of Physiology in University College, Dundee. Communicated by Professor M. FOSTER, Sec.R.S. Received November 19, 1892.

(Abstract.)

1. The assumption that the E.M.F. of the current of rest of the skin of the Fish is entirely due to mucin-metamorphosis, and that it is not possible to attribute it to the presence of glandular elements is negatived, in the case of the Eel, by the absence of any such mucinous change in the superficial epidermic cells and by the presence of abundance of secretory cells throughout the structure.

2. The existence of considerable differences of potential between two contacts upon the outer surface of the skin, and the fact that such E.M.F. is capable of excitatory augmentation upon mechanical stimulation, coincides with the assumption that the E.M.F. of the current of rest is the outcome of glandular processes of variable activity and is not compatible with the theory of origin of the E.M.F. in mucin-metamorphosis.

3. The reductions in the E.M.F. of the normal rest current following exposure of the skin to carbonic acid gas and to the vapour of chloroform, and the subsequent recovery upon admission of air, are strong evidence that the origin of the E.M.F. is in some active vital processes taking place in the skin, and it is reasonable to assume that these occur in its secretory elements.

4. The demonstration that the E.M.F. of the skin of the Eel undergoes an excitatory variation as a result of electrical, thermic, and mechanical stimulation, is in accordance with what is known to occur in other glandular structures, and the fact that such excitatory change manifests itself as a positive variation of the current of rest agrees in the main with the phenomena observed in other cases.

5. The fact that chloroform narcosis excludes the possibility of the excitatory variation upon stimulation, at the same time as it reduces the E.M.F. of the normal rest current to zero, supports the assumption that the E.M.F. of the current of rest and that of the current of action originate in one and the same source.

6. Finally, the reduction of the E.M.F. of the normally directed current of rest by atropinisation and the complete absence of any excitatory variation under such conditions, are facts strongly in favour of the hypothesis that both the E.M.F. of the current of rest and that of the current of action are from a glandular source.

IV. "Preliminary Note on the Relation of the Ungual Corium to the Periosteum of the Ungual Phalanx." By F. A. DIXEY, M.A., M.D., Fellow of Wadham College, Oxford. Communicated by E. A. SCHÄFER, F.R.S. Received November 22, 1892.

The corium underlying the epithelium of the developing nail in the human embryo is at an early age distinguishable from the cutis vera of the remainder of the digit by its greater thickness and density. Opposite the groove across the dorsal surface of the digit, which represents the anterior border of the growing nail, the thick firm connective tissue layer constituting the ungual corium does not thin out or pass into the general corium; but, still preserving its original thickness, it sinks deeply into the substance of the digit, and travers-

ing the loose subcutaneous tissue in the form of a well-defined curved band, with the convexity generally directed forwards, it reaches and becomes continuous with the periosteum surrounding the distal extremity of the ungual phalanx. These two structures, viz., the ungual corium and the periosteum of the ungual phalanx, which are histologically very similar to one another, and distinct from the connective tissue forming the bulk of the terminal segment of the digit, are thus placed in complete continuity by means of the curved band of dense connective tissue above described.

V. "Experiments on the Action of Light on *Bacillus anthracis*." By H. MARSHALL WARD, F.R.S., Professor of Botany, Royal Indian Engineering College. Received December 15, 1892.

It is abundantly evinced by experiments that direct insolation in some way leads to the destruction of spores of *Bacillus anthracis*, and in so far the results merely confirm what had already been discovered by Downes and Blunt in 1877 and 1878.*

From the fact that an apparent retardation of the development of the colonies on plates exposed to light was observed several times under circumstances which suggested a direct inhibitory action of even ordinary day-light, the author went further into this particular question with results as startling as they are important, for if the explanation given of the phenomena observed in the following experiments turns out to be the correct one, we stand face to face with the fact that by far the most potent factor in the purification of the air and rivers of bacteria is the sun-light. The fact that direct sun-light is efficacious as a bactericide has been long suspected, but put forward very vaguely in most cases.

Starting from the observation that a test-tube, or small flask, containing a few c.c. of Thames water with many hundreds of thousands of anthrax spores in it may be entirely rid of living spores by continued exposure daily for a few days to the light of the sun, and that even a few weeks of bright summer day-light—not direct insolation—reduces the number of spores capable of development on gelatine, it seemed worth while to try the effect of direct insolation on plate-cultures, to see if the results could be got more quickly and definitely.†

Preliminary trials with gelatine plate-cultures at the end of the

* See p. 237 of "First Report to the Water Research Committee of the Royal Society" ('Roy. Soc. Proc.,' vol. 51, 1892) for the literature on this subject up to 1891.

† It appears that Buchner ('Centr. f. Bakt.,' vol. 12, 1892) has already done this for typhoid, and finds the direct rays of the summer sun quite effective.

summer soon showed that precautions of several kinds were necessary. The direct exposure of an ordinary plate-culture to the full light of even a September or October sun, especially in the afternoon, usually leads at once to the running and liquefaction of the gelatine, and although the exposed plates eventually showed fewer anthrax colonies than similar plates not exposed, the matter was too complicated to give satisfactory results. Obviously one objection was that the spores might have begun to germinate, and the young colonies killed by the high temperatures.

Experiments made in October with gelatine plates wrapped in black paper, in which a figure—a square, cross, or letter—was cut, also led to results too indefinite for satisfaction, although it was clear in some cases that if the plates lay quite flat, the illuminated area was on the whole clear of colonies, while that part of the plate covered by the paper was full of colonies.

But another source of vexation arose. After the plates had been exposed to the sunlight for, say, six hours, it was necessary to put them in the incubator (20—22° C. was the temperature used) for two days or so, to develop the colonies, and in many cases it was observed that by the time the colonies were sufficiently far advanced to show up clearly, liquefaction had extended so far as to render the figure blurred and doubtful.

Stencil plates of zinc were employed with, at first, equally uncertain results. The stencil plate was fixed to the bottom of the plate culture, outside, and every other part covered with blackened paper: the plate was then placed on a level surface, the stencil-covered face upward, and exposed to the direct sunlight. As before, the gelatine softened and in many cases ran, and the results were uncertain, though not altogether discouraging.

In November it was found that more definite results could be obtained, and the problem was at last solved.

Meanwhile it had already been found possible to obtain sun prints in the following way with agar plates. Ordinary agar was heated and allowed to cool to between 50° and 60° C., and was then richly infected with anthrax spores, and made into plates as usual. Such plates were then covered with a stencil plate on the lower face—the stencil plate being therefore separated from the infected agar only by the glass of the plate—and wrapped elsewhere closely in dull black paper, so that, on exposure to the sun, only the cut-out figure or letter allowed the solar rays to reach the agar.

Such plates were then exposed to the direct rays of the October sun for from two to six hours; or they were placed on the ring of a retort-stand, stencil downwards, and the sun-light reflected upwards from a plane mirror below.

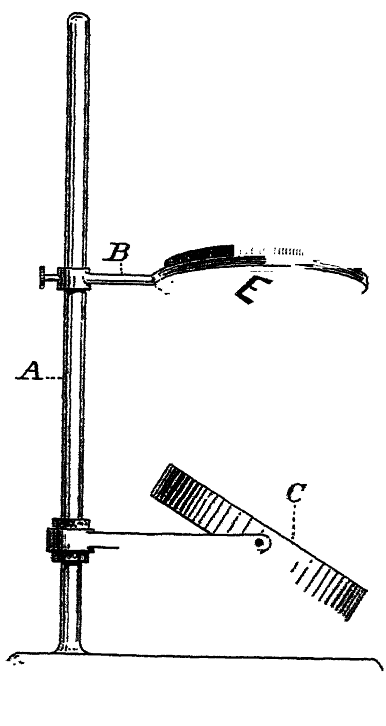
After the insolation, these plates were incubated for at least forty-

eight hours at 20° C., and on removing the wrappers the colonies of anthrax were found densely covering all parts of the plate except the area—a letter or cross, &c.—exposed to the sun-light. There, however, the spores were killed, and the agar remained perfectly clear, showing the form of a sharp transparent letter, cross, &c., in a groundwork rendered cloudy and opaque by the innumerable colonies of anthrax.

Experiments proved that this was not due to high temperature, for a thermometer with its bulb next the insolated glass rarely rose beyond 14° to 16° C., and never beyond 18° C., and even if the thermometer did not record the temperature inside the plate, this can scarcely have been much higher.

As long as this latter point remained uncertain, however, the experiments could not be regarded as satisfactory; whence it was necessary to again have recourse to gelatine cultures. The gelatine employed began to run at 29° C., and in November it was found that such plates exposed outside, either to directly incident sunshine, or to directly reflected rays, showed a temperature of 12° to 13° C. at the insolated glass surface, and even five to six hours' exposure caused no running of the gelatine.

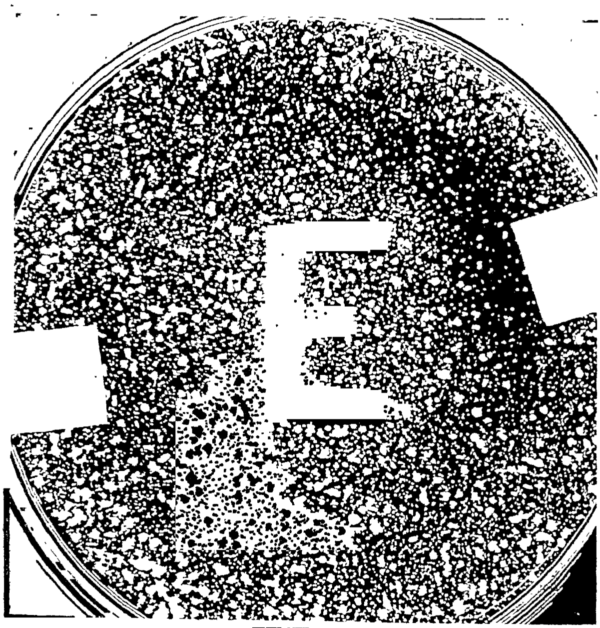
FIG. 1.



The following experiment may be selected as a type of the rest:—A (fig. 1) is the upright of an ordinary retort-stand; on the ring B rested a gelatine plate-culture of anthrax spores, covered with black paper everywhere except the cut-out letter E, seen on its lower face. C was an ordinary plane microscope-mirror, with its arm fitted to a cork on A.

The whole was placed in the middle of a field at Cooper's Hill at 9.30 A.M. on Wednesday, November 30, and exposed to the clear, but low, sunshine which prevailed that day, the mirror being so arranged (from time to time as necessary) as to reflect the light on the E the whole period, until 3.30 P.M., when the plate was removed and placed in the dark incubator at 20° C. On the following Friday—*i.e.*, after less than forty-eight hours' incubation—the letter E stood out sharp and clearly transparent from the faint grey of the rest of the plate of gelatine. Not a trace of anthrax could be found in the clear area, even with the microscope, while the grey and almost opaque appearance of the rest of the plate was due to innumerable colonies of that organism which had developed in the interval.

FIG. 2.



It was impossible to incubate the plate longer for fear of liquefaction, whence the sceptical may reply that the anthrax exposed to light was only retarded; the experiments with agar show that such

is not the case, however, and that if the insolation is complete the spores are rendered incapable of germinating at all, as proved by removing pieces of the clear agar or gelatine and attempting to make tube cultures from them: in all cases where insolation is complete they remain sterile.

The chief value of these gelatine plate exposures in November, however, is that they prove conclusively (1) that the rays of a *winter* sun are capable, even if reflected, of killing the spores, and (2) that it is really the solar rays which do this directly, and not any effect of a higher temperature, since the gelatine remains solid throughout.

Experience has shown, however, that some precautions are necessary in selecting the anthrax cultures employed for these experiments with gelatine. The light certainly retards or kills (according to its intensity or the length of exposure) virulent spores, but if one takes the spores, mixed with vegetative bacilli, direct from a thoroughly liquefied gelatine culture, or from a bouillon culture, the plates are apt to be liquefied too rapidly for the proper development of the light print, evidently because so much of the liquefying enzyme is carried in when inoculating the plates. The same danger is run when active bacilli alone are employed.

The best method of avoiding these disadvantages has been found to be the following, and it has the additional merit of enabling us to prove, beyond all doubt, that the ripe spores of *Bacillus anthracis* are really inhibited or killed by sunlight.

A few c.c. of sterile distilled water in a tube are thoroughly saturated with the anthrax spores taken from an old culture which has never been exposed to light, and the tube placed for twenty-four hours at 56° C.; this kills all immature spores, bacilli, and enzymes, and leaves us with a crop of the most resistant and fully matured virulent spores.

Experiments with such spores have been made to determine the relative power of the different rays of the spectrum to destroy the anthrax.

It is necessary to note first, however, that in experimenting with the electric light, although but few exposures have been made as yet, it is evident that its effects are feebler than those of the winter sun.

At present it has only been possible to observe that the inhibiting effects are stronger at the blue end of the spectrum than at the red, and exposures to sunlight passing through coloured glasses confirm this result; but the observations are being continued in the hope of getting a perfectly sharp record of the effects of each set of rays.

The following series of experiments are quoted in detail, because they teach several details of importance, in addition to proving the main fact.

On December 7 three gelatine plates and five agar plates were

prepared with spores from a very vigorous and virulent agar tube of anthrax. The spores, which were quite mature, were not subjected to heat, but simply shaken in sterile water to wash and separate them thoroughly.

The three gelatine plates were made at 35° C., the agar plates at 60° C., neither of which temperatures could injure the ripe spores.

The three gelatine plates were labelled *p* 1, *p* 2, and *p* 3, and the agar plates *p* 4 to *p* 8 in order.

Immediately after making the plates, all were exposed to the December sun, except plates *p* 4, *p* 5, and *p* 6, and this was done as follows:—In each case the plate had a stencil plate with a cut-out letter on its lower face, and arranged as described above (p. 396).

p 1, a gelatine plate with a *large* letter M, was exposed, face down, to the light reflected from a mirror (see fig. 1) for three hours on December 7, and for four hours on December 8, the interval being passed in a cold room (*t* about 8—9° C.), and then incubated at 20° in the dark.

p 8 was treated in exactly the same manner. But this was an agar plate with a *large* W.

p 2, a gelatine plate with a *large* H, was exposed and heated in the same way, except that no mirror was used, the latter being upwards towards the sun.

p 3, a gelatine plate with a *large* B, was similarly exposed, face up, but a plane mirror arranged to reflect light down upon it.

p 7, an agar plate with a *large* E, was treated exactly as the last.

There now remain the three agar plates, *p* 4, *p* 5, and *p* 6, to account for.

p 4 was placed forthwith in the dark incubator at 20° C.

p 5 and *p* 6 were kept for eighteen hours in a drawer, the average temperature of which is almost 16° C., and were not exposed till next day (December 8), when they lay for five hours, face upwards, and with a mirror above them. *p* 5 had a *small* E, and *p* 6 a broad but small I, to let the light in.

After exposure, these also were put in the same incubator with the others.

Nothing was visible to the unaided eye on these plates (except *p* 4) until the 11th instant, though the microscope showed that germination was proceeding on the 10th. The plate *p* 4, however, had a distinct veil of colonies all over it on the 9th, and this had developed to a dense typical growth by the 11th.

On December 11, at 10 A.M., the state of affairs, as regards the exposed plates, was as follows:—

p 5 and *p* 6 showed each a sharp transparent letter—E and I respectively—of clear agar in a dull grey matrix of strong anthrax colonies, which covered all the unexposed parts of the plate.

p 1, *p* 2, and *p* 3 showed in each case a perfectly clear central patch, about $1\frac{1}{2}$ inches diameter, with anthrax colonies in the gelatine around. These anthrax colonies were the *larger and more vigorous the more distant they were from the clear centre.* In other words, the anthrax spores had begun to germinate, and the colonies were growing more vigorously, in centripetal order.

On *p* 7 and *p* 8 germination was beginning, but the colonies were as yet too young to enable one to judge of the results.

The first point of interest is to account for the pronounced results in the plates *p* 5 and *p* 6, and the want of sharp outlines in *p* 1, *p* 2, and *p* 3, and the explanation seems to be that, owing to the plates 5 and 6 having laid over night at 16° C., the spores began slowly to germinate out, *and were consequently in their most tender condition* when exposed to the sunlight next day.

The peculiar centripetal order of development of the colonies on plates *p* 1, *p* 2, and *p* 3 gave rise to the following attempt at explanation. After observing that the clear space in the middle was not due to the centre of the plate being raised, and the infected gelatine having run down to the periphery—a possible event with some batches of Petrie's dishes—it was surmised that the *large* letters employed might give the clue.

This was found to be the case. The solar rays on entering the plate were largely reflected from the glass lid of the plates, and so produced feebler insolation effects on parts of the plate around the letter: these effects were naturally feebler and feebler towards the margin, and so the inhibitory action became less pronounced at distances further and further removed from the centre. Those spores, therefore, which were nearest the periphery germinated out first, and those nearer the centre were retarded more and more in proportion to their proximity to the insolated letter.

That this is the correct interpretation of the facts follows clearly from the further behaviour of the above plates.

At 10 P.M. on the 11th—*i.e.*, twelve hours after the morning examination—the plates *p* 1, *p* 2, and *p* 3 exhibited their respective letters M, H, and B quite clearly, in the grey matrix of anthrax which had rapidly developed in the interval, and excepting a slight want of sharpness in the H of *p* 2, the results could hardly have been more satisfactory.

In *p* 7 and *p* 8 the *very faint* outlines of the letters were also showing.

On the 12th, at 8.30 A.M., the gelatine plates had begun to run, and although the M of *p* 1 was still intact, and very well marked, *p* 2 had liquefied completely, so that the H was a clear patch with blurred outlines in the centre. *p* 3 still showed the outlines of the B, but it was impossible to keep it longer.

The main point was definitely established, however, and the treatment of the plates proves conclusively that the spores are not killed by high or low temperatures, *but by the direct solar rays.*

These experiments are being continued in order to answer some other questions in this connexion.

The gelatine and agar after such exposures as have been described are still capable of supporting a growth of *B. anthracis* if fresh spores are sown on them, whence the effects described are not merely due to the sub-strata being spoilt as food material.

The Society adjourned over the Christmas Recess to Thursday, January 19, 1893.

Presents, December 15, 1892.

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"On the Method of Examination of Photographic Lenses at the Kew Observatory." By LEONARD DARWIN, Major, late Royal Engineers. Communicated by Captain ABNEY, C.B., F.R.S. Received April 13,—Read June 2, 1892.

The Kew Committee of the Royal Society decided, about a year ago, to undertake the examination of photographic lenses, thus adding one more to the already numerous list of tests and certificates issued by the Kew Observatory. Captain Abney was the member of the Committee who originated the idea, and he was requested to organise the system in detail. This he undertook to do, but unfortunately it proved that official duties and his work in connexion with colour vision, &c., made it impossible for him to give the necessary time to the enquiry. In consequence of this, I was asked by the Kew Committee, with the full acquiescence of Captain Abney, to give my assistance in the matter, which I gladly consented to do; eventually the arrangements devolved almost entirely upon myself, acting in co-operation with Mr. Whipple, the Superintendent of the Observatory, and aided by consultations with Captain Abney; but I should add that as to the arguments and discussion in this paper I alone am responsible. A very considerable amount of time and energy was expended by Mr. Whipple and myself before the final recommendations could be made, but now, since the whole of the proposals have received the provisional approval of the Kew Committee, it is open to anyone to get a photographic lens examined at Kew on payment of a small fee.

It is important first to state clearly the general idea which the Kew Committee had in view when they undertook this new work, for, if the methods adopted are judged from any other standpoint, they will, no doubt, be found open to criticism. The object of the Committee was to organise a system by which any one could obtain, on payment, an impartial and authoritative statement of the quality of a lens to be used for ordinary photographic purposes, and that the fee, which had to cover the cost of the examination, should be moderate. This latter consideration acted as a serious restriction, and it was consequently necessary that all the tests should give results of undoubted practical value to the practical photographer; the certificate of examination must be recorded in the way most

generally useful, and in language which could not fail to be understood. A complete scientific investigation of a lens from every point of view would occupy so long a time as to make the necessary fee quite prohibitive, and, moreover, the results would contain much information which would be quite useless to the ordinary user of the lens.

There are undoubted advantages in testing a lens by the examination of negatives made by it, but it may be here stated, once for all, that the question of expense rendered it impossible, for the present, to adopt any photographic method; eye observations alone have to be relied on.

The form of entry is made to state for what special purposes the lens is intended, whether for portrait work, for landscape views, or for copying plans, &c. Every lens for photographic purposes is more or less of a compromise. Great rapidity, great perfection in definition, and power of covering very wide angles are incompatible qualities, and one or other of them must be sacrificed. It is therefore evidently unfair to expect different types of lenses to give equally good results under the same test; for if we select a lens excelling greatly in one of these qualifications, we must deliberately abandon the expectation of its attaining the highest standards in the others. For example, in a portrait lens great rapidity is required, but, on the other hand, a less high standard of definition near the edges of the plate can be tolerated than with a landscape lens. No opinion could possibly be expressed at Kew as to the wisdom of demanding extra perfection or powers in any respect, and it is therefore necessary that the lenses should be, to a certain extent, classified by the parties sending them in for examination.

The smaller the aperture of a lens, the larger will be the field of sharp definition covered by it, and a complete study of a lens would tell us the size of the plate which is properly covered when each of the different stops is used. Considering the restrictions necessarily imposed on the work, such a lengthy examination could not possibly be thought of. Hence, when discussing the programme of tests to be applied at Kew, it was soon evident that the time devoted to the examination of each lens had to be limited by making the person entering it state either the number of the largest stop by which it should be judged, or the size of the plate for which it would be used; on the first supposition the Kew certificate would have given the size of the plate which the lens covered satisfactorily with the named stop; and, on the second supposition, it would have indicated the size of the largest stop that could be used to give results up to a certain standard, or the rapidity of the lens in normal cases when used for the plate of the named size. The latter of these two alternatives has been adopted, because it is considered that the owner or intending purchaser of the lens will, in most cases, have already

decided on the size of the plate he intends to use, and that what he wants to know is whether it is suitable for that plate or not. When further information is desired, the lens may be entered for examination for two or more sizes of plates.

The following is an example of the Certificate of Examination, the part in italics representing the result of the testing of the lens. Reference is made to the pages where a detailed discussion on each test can be found:—

KEW OBSERVATORY, RICHMOND, SURREY.

Certificate of Examination of a Photographic Lens.

1. Number on lens, *3876*. Registered number, *95*.
2. Description, *landscape lens*. Diameter, *1·5* inches.
3. Maker's name, *A. B.*
4. Size of plate for which the lens is to be examined, *6·5* inches by *8·5* inches.
5. Number of reflecting surfaces, *4* (see p. 409).
6. Centering in mount, *good* (see p. 409).
7. Visible defects—such as striæ, veins, feathers, &c., *nil* (see p. 409).
8. Flare spot, *nil* (see p. 409).
9. Effective aperture of stops (see p. 412)—

Number engraved on stop.	Effective aperture. Inches.	*f*/number.	C.I. No.*
No. *7·5*	*1·32*	*f/8·6*	*1/1·38*
No. *10*	*1·19*	*f/9·5*	*1/1·12*
No. *15*	*0·97*	*f/11·7*	*1·35*
No. *25*	*0·75*	*f/15·1*	*2·26*
No. *50*	*0·49*	*f/23*	*5·3*
No........	....	....	....
No........	....	....	....

10. Angle of cone of illumination with largest stop = *68°*, giving a circular image on the plate of † *13·2* inches diameter.

    Angle of cone outside which the aperture begins to be eclipsed, with stop C.I. No. *1/1·38*, = *20°*, giving a circular image on the plate of *4·0* inches diameter.

    Diagonal of the plate = *10·7* inches, requiring a field of *51°*.

    Stop C.I. No. *5·3* is the largest stop of which the whole opening can be seen from the whole of the plate (see p. 414).
11. Principal focal length,† = *11·28* inches. Back focus, or length from the principal focus to the nearest point on the surface of the lenses, = *10·4* inches (see p. 418).
12. Curvature of the field, or of the principal focal surface. After focussing † the plate at its centre, movement necessary to bring it into focus for an image *1·5* inches from its centre = *0·02* inch.

* C.I.—International Congress System. [See p. 413; an explanatory note will accompany certificate.]

† The lens is focussed on a very distant object.

Ditto for an object *3* inches from its centre = *0·04* inch.
„ *4·5* „ = *0·10* „
„ *5* „ = *0·15* „ (see p. 425).

13. Definition at the centre with the largest stop, *excellent.* C.I. stop No. *1·35* gives *good* definition over the whole of a *6·5* inch by *8·5* inch plate (see p. 429).
14. Distortion. Deflection or sag in the image of a straight line which, if there were no distortion, would run from corner to corner along the longest side of a *6·5* inch by *8·5* inch plate = +0·*01* inch* (see p. 436).
15. Achromatism. After focussing† in the centre of the field in white light, the movement necessary to bring the plate into focus in blue light (dominant wave-length, 4420), = +0·*04* inch.‡ Ditto in red light (dominant wave-length, 6250) = −0·*01* inch† (see p. 440).
16. Astigmatism.§ Approximate diameter of disc of diffusion† in the image of a point, with C.I. stop No. —— at —— inches from the centre of the plate = 0·—— inch (see p. 443).
17. Illumination of the field. The figures indicate the relative intensity at different parts of the plate.†

With C.I. stop No. *1/1·38*.			With stop No. *5·3*.	
At the centre	100	:	Ditto	100
At *3* inches from the centre	*67*	:	Ditto	*82*
At *5·35* „	*28*	:	Ditto	*66*

General Remarks.—*An excellent medium angle rapid objective, practically free from distortion.*

Date of issue ____________

W. HUGO, Observer.
G. M. WHIPPLE, Superintendent.

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* The sag or sagitta here given is considered positive if the curve is convex towards the centre of the plate.

† The lens is focussed on a very distant object.

‡ Positive if movement towards the lens, negative if away from it.

§ The lens is supposed to be perfect in other respects.

Note.—The following is the scale of terms used: excellent, good, fair, indifferent, bad.

In considering and in recording the results of examinations, it has been found convenient to give more exact meanings to certain expressions than have as yet been assigned to them. The following definitions have therefore been adopted at Kew:—

A narrow angle lens means one covering effectively not more than 35°.

A medium angle lens means one covering between 35° and 55°.

A wide angle lens means one covering between 55° and 75°.

An extra wide angle lens means one covering more than 75°.

With regard to the wording of the "General Remarks" in the certificate, it should be remembered that the lens is judged entirely with reference to a plate of named size; the lens is therefore classed as above by the angle of field which is given as the last item but one in test No. 10. If the same lens is examined for plates of different sizes, the certificate would be worded differently in each case.

The C.I. No. of a stop means the number which indicates the intensity of illumination produced by it on the plate according to the system proposed at the International Photographic Congress of 1889 (see p. 413).

The *largest normal stop* means the largest stop that can be used with the lens so as to produce definition up to a selected standard of excellence all over a plate of given size, the objects whose images are seen being all equally distant.

A slow lens means one of which the largest normal stop has a less diameter than has C.I. No. 6.

A moderately rapid lens is one of which the largest normal stop is C.I. No. 6, or larger than that size and less than C.I. No. 2.

A rapid lens is one of which the largest normal stop is C.I. No. 2, or larger than that size and less than C.I. No. 2/3.

An extra rapid lens is one of which the largest normal stop is C.I. No. 2/3, or larger than that size.

For convenience of reference, these definitions will in future accompany the certificate, probably in the form of additional notes.

No doubt most lenses are supplied with stops larger than the ones here called the largest normal stops, even if it is not intended to use smaller plates than those under consideration; this is, of course, very right, for in many cases the photographer will be willing to sacrifice the definition near the edge of the plate for the sake of increased rapidity.

It now remains to be shown in what way the above certificate of examination would be useful to the practical photographer, who has sent his lens to Kew for the purpose of being tested. It may, we think, be assumed that he wants answers to the three following questions:—1st. Is the lens a good one? 2ndly. Does it properly cover the plate of the named size? And 3rdly. What exposure must be given when using the different stops?

With regard to the two first questions, the result of the examination is recorded in such a way that he may either rely on the "General Remarks," or he may form an independent judgment from the results of the tests.

In order to decide himself, from the records in the certificate, whether the lens is generally speaking a good one, he should first look to test No. 13 to see if the definition in the centre of the plate with the largest stop, is "excellent," as should always be the case; he should then consider test No. 15, by which he will see what are the faults introduced by the lens not being properly corrected for chromatic aberration. With regard to the second question, that is to say, when considering whether the plate he intends to use is properly covered or not, he should chiefly look to the results recorded under test No. 13, where is given the size of stop or the rapidity of

the lens for a given standard of definition up to the edge of the plate; if the definition at the centre is "excellent," then any want of definition at the margin will be chiefly due to curvature of the focal surface, or to astigmatism, and therefore the results of tests Nos. 12 and 16 should be considered at the same time as test No. 13. He must also look carefully to the result of test No. 14, which shows the maximum distortion produced in the image; it will depend for what class of work the lens is to be used whether he should consider the amount of curvature in the image of a straight line near the edge of the plate, which will be there indicated, is objectionable or not.

The "General Remarks" are recorded as the result of exactly similar considerations to those discussed above, the experience gained by the examination of lenses of undoubted quality giving an idea of what standard of excellence should be required.

With regard to the third question, as to the exposure to be given with the different stops, it may be hoped before long that the C.I. numbering will be generally adopted by all practical photographers, in which case the results of test No. 9 will give the information required.

In many works on photography the view is expressed that the practical photographer also wishes to know from what point on his lens he should measure or adjust the distance of any object so that by reference to tables, he can obtain definite enlargements or reductions; this is, in fact, urging that the position of the principal planes should be marked on the mounting of all lenses. According to our experience this is a want in reality very seldom felt in practice. The tables are, no doubt, sometimes used to get approximate results, the fine adjustment of scale being afterwards done by measurements on the ground glass; but if the slot between the two lenses of a doublet is used as the point from which the measurements of distance are made, the results will be quite near enough to the truth to serve as a first adjustment, and for this purpose nothing will be gained by marking the exact position of the principal planes; it should, however, be stated that the omission to mark them is merely made in consequence of the necessity felt of minimising in every possible direction the time spent in the examination.

Each test to which the lens is subjected will now be described in detail, together with such discussion as to the reason for its adoption as may appear necessary.

The first four headings of the certificate deal with the numbering of the lens, the maker's name, the size of plate for which the lens is to be examined, &c.; and, as these do not form part of the results of the examination, no remarks are necessary with regard to them.

5. *Number of Reflecting Surfaces.*

In most cases the number of reflecting surfaces of glass is known at once from the type of lens, but, if in doubt, a simple experiment will settle the point; the room is darkened, and the reflection of a lamp is observed in the lenses; each of the surfaces of the lenses will give one direct reflected image, and the number can thus easily be counted. The amount of light which reaches the photographic plate decreases with an increased number of lenses, because of this reflection, and this circumstance should not be forgotten in estimating the suitability of a lens for any special purpose. Surfaces merely separated by Canada balsam reflect little light, and need not be considered from this point of view.

6. *Centering in Mount.*

Two different errors might be described under this heading: either (1) the optical axis of a perfect lens may not coincide with the axis of the mounting, or (2) the axes of the different lenses of a doublet or triplet may not all be in the same straight line. As to the first of these errors, we believe it would never be sufficient to have any appreciable effect on the practical value of a lens, and therefore no test for it is considered necessary. With regard to the second error, Wollaston's test is the only one applied; this consists of looking at the flame of a lamp or candle *through* a compound lens, and noting if all the different images of the light as seen by successive reflections from the surfaces of the glass can be brought into line by a suitable movement of the whole lens, which should be the case if the component lenses are arranged about a common axis.

It may be remarked that the nodal points may be shifted away from the mechanical axis of the lens in consequence of either of the above-mentioned errors, and also, on the other hand, that the second error may exist—that the axes of the component lenses of a doublet may not be coincident—and yet one or both of the nodal points may conceivably be found on the mechanical axis of the mounting; it follows, therefore, that to estimate the distance between the nodal points and the mechanical axis, which has been suggested as a means of detecting any want of centering, does not answer that purpose very well.

7. *Visible Defects, such as Striæ, Veins, Feathers, &c.*

Under this heading any faults detected by a careful inspection are given.

8. *Flare Spot.*

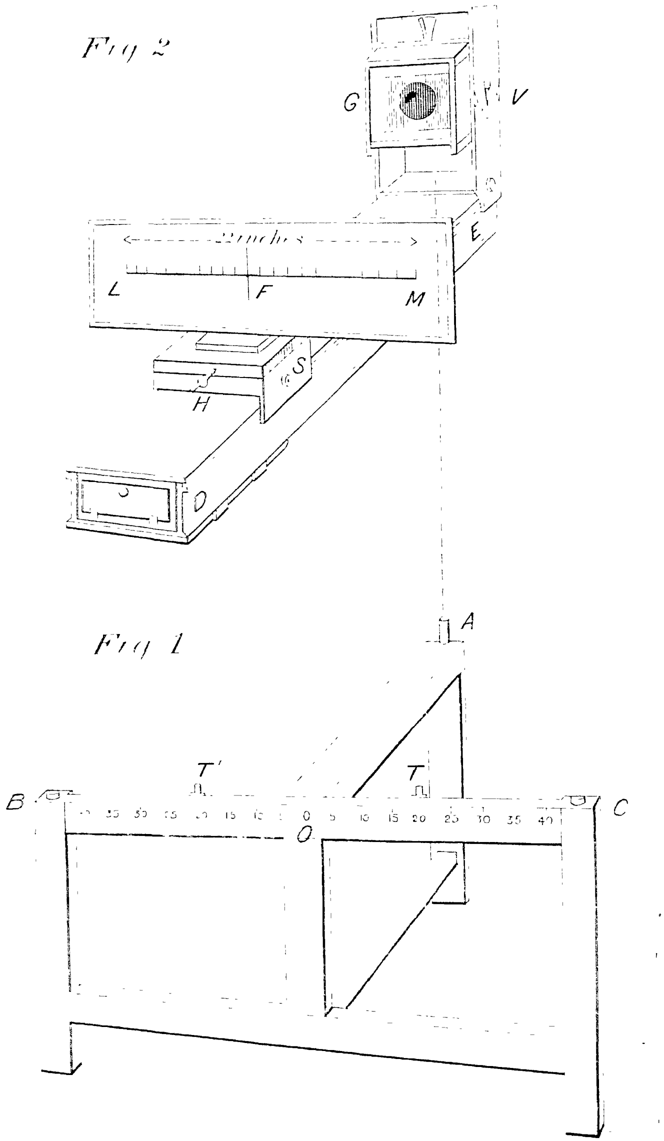
The defect known as *flare spot* consists of a bright spot or patch of light being formed in the centre of the field. To detect it, the lens is

placed in an ordinary camera, which should be pointed at the sky; if the ground glass is brought to the principal focus, the flare spot is then readily visible.

For tests Nos. 9 to 16 an apparatus designed by myself, and which I have called the "testing camera," is used. It is neither an expensive nor an elaborate contrivance, and there can be no doubt that if more money had been expended a more perfect machine could have been made. Until a system of this sort has been in regular use for some time, and until it has stood the fire of criticism, experience shows, we think, that the apparatus employed is apt to be little more than a good working model of what it will become by future developments; but improvements would in this instance probably tend to increased rapidity rather than to increased accuracy, for the results obtained are now quite accurate enough for all practical purposes. Even now alterations are under consideration, such as the substitution of a sliding eye-piece on a graduated bar for the long sheet of ground glass. For the above-mentioned reasons, and because much expenditure could not be justified until it was certain that lenses would be sent for examination in considerable numbers, the Kew Committee raised no objection to the somewhat make-shift appearance of the apparatus.

The general idea of the testing camera is extremely simple, but the name perhaps is hardly a happy one, as there is no "camera" or chamber about it. Except for the absence of bellows, it may be said to consist of the essentials of an ordinary camera, which is capable of being revolved horizontally about a vertical axis passing through the lens; though it must be confessed that this description gives no idea of its appearance. The three-legged stool or bench, seen in fig. 1, represents the legs of the camera, and fig. 2 shows the apparatus that takes the place of the body; G is the lens-holder, and LM the ground glass, both of which are capable of independent movement backwards and forwards on the hollow wooden beam DE, called the "swinging beam." There is a conical brass cap or pivot, not shown in the sketch, under the upper plank of the swinging beam, underneath where the lens-holder G is shown in the sketch. The whole of the apparatus shown in fig. 2 is placed on the top of the three-legged stool, the round-headed iron pin A passing loosely through a hole in the lower plank of the swinging beam, and fitting into the conical brass cap or pivot. The swinging beam, being thus supported by the pin A and by the long arm BC of the stool, is capable of being revolved round A as a centre. On the ground glass is engraved a horizontal line, which is accurately divided into fiftieths of an inch; this line passes through the centre of the ground glass (or through the point where the perpendicular from the lens-holder cuts the glass), and is also parallel to BC, the top of the stool on

FIGS. 1 AND 2.



which the swinging beam slides, when the camera is in position; thus the image of an object will appear to run along the scale as the swing-

ing bar is moved from side to side. The ground glass can be brought approximately into focus by means of the already-mentioned movement to and fro on the swinging beam, but for accurate adjustment a slow motion arrangement is attached to the movable part itself; the handle H gives the required motion, and there is a scale S, called the "focus scale," by means of which these small movements can be accurately measured. On the lens-holder there is a movement, corresponding to the swingback of an ordinary camera, by which the lens can be made to revolve vertically round a horizontal axis, without, of course, any corresponding movement of the ground glass; there is a vertical arc, V, by means of which we can read off the vertical angles through which the lens is rotated. An arrangement is also suppled by means of which the lens can be moved backwards and forwards *on* the movable stand, thus allowing the position of the lens to be so adjusted that the horizontal axis can be made to pass through any point in its axis.

9. *Effective Aperture of Stops.*

Number engraved on stop.	Effective aperture. Inches.	*f*/number.	C.I. No.
No.........	....	....	....
No.........	....	....	....
No.........	....	....	....
No.........	....	....	....
No.........	....	....	....
No.........	....	....	....
No.........	....	....	....

The effective aperture of one or more of the various stops supplied with the lens is found by a well-known method. The image of a very distant object is first brought into focus on the ground glass of the testing camera; a collimator, which has itself been previously focussed on a distant object, may be used instead of the distant object; the ground glass is then taken out and exactly replaced by a tin plate with a small hole at the centre; this hole, which should be very small, will, therefore, be at the principal focus of the lens. The room being darkened, a gas burner is placed behind the small hole, and thus parallel rays, in the form of a cylinder, are made to issue from the lens towards the front. A piece of ground glass, with a graduated scale engraved on it, is now held in front of the lens, and the diameter of the illuminated disc, or section of the cylinder as seen on the glass, is directly measured off as any stop is inserted in its place. Thus is found the

effective aperture of the largest stop, as recorded in the Kew Certificate of Examination. The ratio of the effective aperture to the diameter is the same for all stops of the same lens, and the effective aperture of the other stops is either measured as above, or calculated from the ratio thus found. As the rays are parallel when emerging from the lens, it is evident that, if the stop is in front of all the lenses, the effective aperture will be the same as the diameter of the stop itself.

By imagining the path of the rays in the above experiment as being reversed, in which case the rays forming the cylinder are all brought to a focus on the plate, it is evident that the intensity of illumination of the plate at the centre, when focussed for distant objects, varies directly as the sectional area of the cylinder, and therefore as the square of the effective aperture as above measured. The intensity of illumination of the plate also varies inversely as the square of the distance from the point in the lens from which all the light may be supposed to emanate, that is, from the nodal point of emergence. If we adopt as our definition of the principal focal length (f) of the lens the length between the principal focus and the nodal point of emergence, it is then evident that the square of the effective aperture divided by f^2 will be a measure of the illumination of the plate. In consequence of this fact, it has for a long time been customary to speak of the diameter of stops in terms of the ratio of their effective apertures to the focal length of the lens; for example, a lens having a stop with an effective aperture equal to one-tenth of its principal focal length is commonly spoken of as working with an intensity of $f/10$. But it has recently been found by photographers that it is practically useful to adopt a definite standard or unit of intensity of illumination in order that the different stops may be numbered in such a way as to readily indicate the different exposures required with each; many systems of this kind have been considered: $f/100$, $f/10$, $f/4$, and $f/\sqrt{10}$, each having been at various times proposed as the basis of enumeration, the numbering of the stops sometimes increasing and sometimes diminishing as the necessary exposure increases. Each of these systems has met with considerable opposition from different quarters; but this is not the place to discuss their relative merits, more especially as in selecting one of them for the Kew certificates, we have been chiefly influenced by considering which has received the sanction of the most authoritative body, and which, therefore, stands the best chance of universal adoption. Judged by this standard, there can be no doubt that the recomendations of the International Photographic Congress of Paris of 1889, as endorsed by the Congress at Brussels, should be adhered to as far as possible.

The following system, which we have called the C.I. system, has therefore been adopted at Kew. The stop $f/10$, the effective aperture

of which is one-tenth of the principal focal length of the lens, is called stop No. 1; and the exposure necessary for any subject with that stop is the unit of exposure for that subject. The other stops are numbered in the inverse ratio of the area of their effective apertures to the area of the effective aperture of stop No. 1. Thus stop No. 2 gives half the intensity of illumination of stop No. 1; and, in any case, to find the time of exposure necessary to produce the same result as with the unit of exposure with stop No. 1, we multiply that unit by the number of the stop in use. The practical rule to find the C.I. number of a stop is to divide the square of the principal focal length by 100 times the square of the diameter of the effective aperture of the stop. The principal focal length, which we require to know in order to calculate the numbering of the stops, is found by test No. 11.

The difficulty of introducing the C.I. numbering of stops will perhaps be greater in England than on the Continent, partly because, previous to the Paris Congress, the Photographic Society of Great Britain had given provisional support to another system based on $f/4$ as a unit. The Photographic Society has been waiting for the recently published reports of the Brussels Congress to reconsider this matter, and it may be hoped that they will join in the effort to get the C.I. system universally adopted, notwithstanding the inconvenience that must be severely felt at first by those who are therefore obliged to change their methods.

10. *Angle of Cone of Illumination with Largest Stop = —— °, giving a Circular Image on the Plate of —— inches diameter. Angle of Cone outside which the Aperture begins to be Eclipsed with Stop C.I. No. —— = —— °, giving a Circle on the Plate of —— inches diameter.*

Diagonal of Plate = —— inches, requiring a Field of —— ° ($= 2\phi$).
Stop C.I. No. —— is the Largest Stop the whole of the Opening in which can be seen from the whole of the Plate.

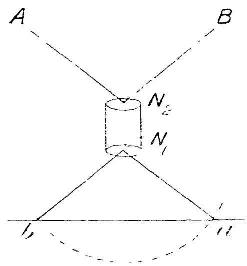
If a stop, or thin metal diaphragm with a circular aperture, is revolved round any axis passing through its plane, and if it is regarded from a little distance, the whole aperture, foreshortened of course, can be seen except in one position in each half revolution; if in a similar way a piece of tubing is revolved about an axis at right angles to its own axis, there is only one position in which the whole of the aperture can be seen, and any movement from this one position will cause the opening to begin to be eclipsed, thus giving it a lozenge-shaped appearance; as the movement goes on, this opening will get smaller and smaller till it is quite obliterated. In looking through a lens as it is revolved about an

axis perpendicular to its own axis, it will be seen that, as a rule, something between these two extremes occurs; commencing from a position when we are looking directly along the axis, no other result than foreshortening the opening is at first produced by the revolution of the lens; then comes an angle at which the aperture in the stop begins to be eclipsed, either by the mounting of the lenses, or by fixed diaphragms, &c.; lastly, we come to an angle at which the lozenge-shaped opening appears to vanish, and no light is seen to come from the lens. It is obvious that the intensity of illumination of different parts of the photographic plate varies with the size of the aperture visible from each point; and, neglecting other considerations for the present, there is thus an inner cone, forming a disc where it cuts the plate, in which the illumination decreases regularly from the centre outwards according to a known law: and there is an outer cone, forming an annulus between where it and the inner cone cut the plate, in which the illumination decreases more rapidly than according to the above-mentioned law; very rapidly, therefore, probably irregularly, on account of the aperture of the stop being successively eclipsed by different parts of the mounting, and certainly according to no law that can be readily stated or ascertained. The test now under consideration gives the angles of these two cones.

The outer cone, which we have called the "cone of illumination," gives the extreme angle of the field of the lens without regard to definition, and is what is known to French authors as the *champ de visibilité*. To find the angle of the cone of illumination, the lens is placed in the testing camera, and the observer looks through the small hole in a sheet of tin plate with which the ground glass has been replaced, as in the last test; the lens-holder is made to revolve about its horizontal axis, and as the axis of the lens moves away from zero, first in one direction and then in the other, the positions at which all light appears to be cut off are noted; the angle between these two positions as read on the vertical arc, V, gives the angle of the cone of illumination.

In order to ensure correct results it is necessary that the axis of rotation should pass through the nodal point of emergence. If in fig. 3 AN_2N_1a and BN_2N_1b represent the extreme rays forming the cone, N_2 and N_1 being the nodal points, it is evident that in order to measure the angle bN_1a of the cone the lens must be revolved about N_1, the nodal point of emergence, as a centre. The necessary adjustment is made in the following manner:—The image of a distant object having been thrown on the ground glass, the lens is turned through a small angle about the horizontal axis, the glass remaining stationary. If the movement of the lens gives rise to any movement in the image, then the axis does not pass through the nodal point of emergence

FIG. 3.



and an adjustment is necessary; this is done by moving the lens-holder in or out, thus making the axis of rotation pass through different parts of the axis of the lens, until the image ceases to show any movement; and this can only be the case when the axis of rotation does pass through the nodal point of emergence. As far as the above considerations are concerned, it is immaterial how far off the small hole in the tin plate is from the lens, but if the horizontal axis has not been made to pass accurately through the nodal point of emergence, this want of adjustment will have much the same effect as a small vertical movement between the two readings of the vertical arc. It is evident that the angular error thus produced will diminish as the distance of the point of observation increases; moreover, any distortion at the edge of the plate will make the above theoretical considerations no longer strictly applicable, and will have the same effect as the axis of rotation not accurately passing through the nodal point. In order, therefore, to minimise these sources of error, the tin plate with the hole in it is removed as far as practicable from the lens before the observation is made.

The angle of the inner cone, that is, of the cone outside which the opening of the stop is partially eclipsed by the mounting of the lens, &c., is measured in the same way as above described for the outer cone, and with the same precautions. When looking through the small hole, the positions on each side of zero at which the aperture begins to be shut off, and beyond which it no longer appears as a perfect ellipse, are easily seen, and the angle between these two positions as measured on the vertical arc gives the angle required. The angles of these two cones are generally given when the observation is made with the largest stop supplied with the lens.

The results of these measurements should be considered in connexion with test No. 17, under which heading the general question of the illumination of the field will be discussed. In order to facilitate the consideration of the covering power of the lens, the diameters of

the circles which these cones make by cutting the photographic plate, when the focus is adjusted for distant objects, are given in the Certificate of Examination. Having found the principal focal length in the manner to be described immediately, the size of these circles can readily be ascertained by a simple graphical method, which is hardly worth describing in detail.

In connexion with this test it may be convenient to adopt the use of the term *angle of field under examination*, (denoted in this paper by 2ϕ,) to signify the angle subtended at the nodal point of emergence by a diagonal of the plate, or the greatest angular distance which could be included in the photograph, supposing the focus to be taken on a distant object. This angle is found by the graphical method mentioned above for determining the diameter of the circles on the plate, and the result is entered on the Certificate of Examination.

If the illumination of the field is not to fall off rapidly towards the edges of the plate, for the normal use of the lens we should employ a stop which covers (or nearly covers) the plate of the given size with its inner cone; that is to say, we should use a stop not larger than the largest stop the whole of the opening in which can be seen from the whole of the plate. In order to find the largest stop which fulfils the above conditions, the lens is revolved about the horizontal axis until the vertical arc reads half the angle of field under examination, and then the different stops are put in one by one until the largest one is found which is seen not to be eclipsed when the observation is made through the hole in the tin plate. The number of this stop is recorded in the certificate.

The readings taken when measuring the angles of these cones are also utilised for the purpose of adjusting the position of the lens in a manner necessary to ensure accuracy in several of the following tests. The vertical arc is so arranged that it reads zero when the axis of the lens is horizontal, that is to say, when the axis passes through the small hole in the tin plate from which the observation is made; hence the two readings on the arc when the lens is revolved about the vertical axis, first one way and then the other, so as just to cut off all the transmitted light, should be exactly the same; if they are not identical the lens-holder is placed in such a position that the reading on the vertical arc is equal to half the difference between them; then it is evident that the mechanical axis of the objective passes through the small hole, or at all events, cuts the tin plate on the same level as the hole. Now this small hole in the tin plate is in the same position as the centre of the engraved line when the ground glass is in position. Hence, this adjustment being made, in future tests we may consider that the mechanical axis of the lens cuts the line on the ground glass near its centre.

11. *Principal focal length* = —— *ins.*
Back focus, or length from the principal focus to the nearest point on the surface of the lenses = —— *ins.*

The following is the method of finding the principal focal length with the testing camera. By means of the mark 0 (see fig. 1), on the three-legged stool, the swinging beam can be brought approximately to a central position; there are also two iron stops, T and T', removable when not wanted, which, when in position, prevent the swinging beam from passing beyond these points. These stops (or, more accurately, the iron plates on the swinging beam with which they come in contact) are capable of adjustment, and thus a means is obtained of allowing the beam to be revolved about A as a centre, through a known angle, with great ease and accuracy. After the focus has been very carefully adjusted for a distant object, and after the beam has been brought approximately to the central position by means of the mark 0 on the stool, the image either of some well-defined object seen through a hole in the window-shutters, or of a mark in the collimating telescope, is made to appear on the centre of the engraved line on the ground glass; this can be done by raising or lowering one or more of the legs of the stool or by moving it laterally; this adjustment being accurately made, the line joining F, the centre of the ground glass, and the centre of the lens, if prolonged, will pass through the distant mark; when once made, this adjustment will hold good, with sufficient accuracy, for all lenses which may subsequently be placed in the testing camera. Now, when the swinging beam is moved from side to side, the image appears to run along the engraved line on the ground glass; the position of the image is first noted when the beam is in contact with the stop T, and afterwards when in contact with the stop T'; twice the distance, as measured on the scale, between these two points gives the principal focal length of the lens under examination.

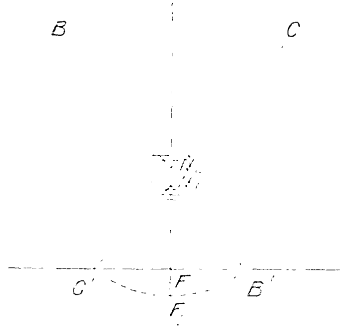
In order to ensure accuracy, certain precautions must be taken. The object must be so far off that the distance between its focus and the focus of a point in the same direction at an infinite distance is considerably less than the probable error of observation. The chief difficulty of finding the principal focal length, in the Kew method, and, indeed, in all methods, consists in obtaining an accurate adjustment for focus; and since, for a given error in focus, the greater the aperture the more diffusion there is in the image, the largest stop should always be used when focussing; but there is no objection to slipping in a smaller stop after the focus is taken so as to obtain as sharp an image as possible, and thus make it easier to read the position on the scale with accuracy.

Before proving that the result above obtained is, in fact, the prin-

cipal focal length of the lens, it may be as well to give a rigid definition of what is here meant by that expression, as it has often been used in somewhat different significations. The definition here adopted of the principal focal length is the distance between the principal focus for visual rays (or the image as seen by the eye of an infinitely distant point on the axis of the lens) and the nodal point of emergence. The use of the term *nodal point* is, perhaps, open to criticism; under the ordinary circumstances of a photographic lens the nodal points and the principal points occupy the same positions, and therefore either of these expressions might have been used in the definition; but if we take into consideration any imaginary circumstances when these two points would not be identical, as, for instance, if one end of the lens was immersed in water, it will be observed that the Kew method of determining the principal focal length would find the distance between the nodal point and the ground glass, and not that from the principal point; moreover, under these imaginary conditions, it would be the distance of the nodal point from the plate which would chiefly be of value to the aquatic photographer, for the intensity of illumination of his plate would vary as the square of that distance and not of the distance from the principal point. But it must be confessed that the term was, in reality, adopted because it is that best known in the photographic world, and not on account of such hair-splitting reasons as these.

It now remains to be seen if the Kew method does give the true principal focal length according to the above definition. In fig. 4, let

FIG. 4.



B, A, and C be three very distant points, A being on the axis of the lens, and B and C being at equal angular distances on either side of it; let N_1 and N_2 be the nodal points; let C′, F, and B′ be the images

of these three points on the ground glass, when, if the distance N_2A is great enough, F will not be further from the principal focus than the error of observation, and may, therefore, be confounded with it. The angle, BN_2C, subtended by the points B and C at the lens, can easily be measured, and, since the incident and emergent rays passing through the nodal points are parallel to each other, the angle $C'N_1B'$ is thus obtained; the distance, $C'B'$, that is, the distance between the images of the two outside points, can be also measured on the ground glass: $C'B'$ and $C'N_1B'$ being given, FN_1 can therefore be found; for since, by supposition, the line AN_2 bisects the angle BN_2C, FN_1 is equal to $C'B'/2 \cot C'N_1B'/2$. This, therefore, is a method by which the principal focal length, as defined above, can be measured. But if, instead of having objects at known angles, only one object is observed, and the camera is revolved round the point N_1, through the angle $C'N_1B'$ between the observations, exactly the same result can be obtained; this is the method adopted at Kew. The movement in parallax of the point N_2 is so small that it may be neglected. The advantage of this method is that a collimating telescope can be used as the object, and thus, during dull weather, the work can be carried on indoors. In working with the testing camera, the angle $C'N_1B'$ represents the angle through which the swinging beam is revolved about the vertical pivot; the stops are arranged so that $C'N_1F = \tan^{-1} \frac{1}{4}$, that is, so that $C'B' = 2\ FN_1$; and, therefore, twice the distance $C'B'$ measured on the ground glass gives FN_1, the principal focal length of the lens. The Kew method, therefore, gives the result required.

It might at first sight appear that a considerable error would be due to the fact that the adjustment to the central position is merely made by a rough mark, and that it is only the total angle $C'N_1B'$ (that is, the angle moved by the swinging beam between the iron stops) which is accurately known. It is true that it can only be said that $C'N_1F$ is approximately equal to FN_1B'; but if $C'N_1B'$ is less than 90°, and if the line N_1F does not differ in direction from the true central position by more than 1°, then the principal focal length obtained in this manner does not differ from the truth, for this reason, by more than 1/17th per cent. As it is considered that this would represent an extreme case, it is therefore evident that this is a negligible source of error.

In order that the Kew method of finding the principal focal length should not be open to any criticism on theoretical grounds, three conditions must be fulfilled: it is obvious that these conditions need not hold good further from the axis of the lens than the points at which the observations were made. 1st. The principal focal surface, or the locus of the focus for very distant objects, must be a plane. 2nd. The image must not be distorted. 3rd. The nodal point of

emergence for visual rays should be the same as the nodal point for actinic rays.

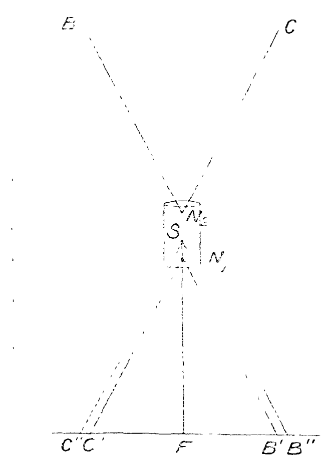
In no lens are these conditions perfectly fulfilled, but before discussing the nature of the errors thus introduced it may be as well to consider shortly for what purposes and with what degree of accuracy the practical photographer wants to know the focal length of his lens. Two uses to which this knowledge would or could be put have already been mentioned, and we know of no others. In the first place, it has been shown how the numbering of the stops depends on the focal length, and how advantageous is the knowledge of the intensity of the illumination of the plate which may thus be gained. But as, on account of the difference in the amount of reflection and absorption of the lenses, two lenses with the same C.I. number of stop may differ more than 10 per cent. in the intensity of illumination in the centre of the field, as in the same objective the difference of illumination of different parts of the field is generally more than 20 per cent., and as the photographer is seldom able to estimate his unit of exposure within this latter percentage, it can hardly be seriously contended that the focal length must be known with very great accuracy for this purpose. The second object for which the photographer may require to know the focal length is for the use of the tables in which the distance is given at which the object has to be placed to obtain a given enlargement or reduction; it has already been stated that this is not, we believe, a want often felt, except for getting approximate results; but if the focal length is used for final adjustments in this manner, it should be known with very considerable accuracy.

With regard to the first condition, as to the focal surface being a plane, it should first be stated that it is found convenient at Kew to bring the ground glass into focus when the swinging beam is in contact with one of the stops, thus insuring the greatest sharpness of image at the points of observation; that is to say, in fig. 4, the principal focal surface is made to pass through the points B' and C', and, if it is not a plane, it may be represented by the dotted curve $C'F_2B'$. Under these circumstances, therefore, the principal focus will be at F_2, and N_1F_2 will represent the principal focal length according to our definition; but it has been shown that the observation gives N_1F as the focal length, thus introducing an error equal to FF_2 in the result. It is to be observed, however, that with a lens giving a markedly curved focal surface, the photographer, in order to get a general minimum amount of diffusion, would adjust his focus by looking at the image at a point somewhat more than half way from the centre to the margin of his plate: for example, with a lens covering 50° or 60°, he would focus at a point some 15° from the centre, or at about the position where the Kew observation for the

focal length is taken; thus, with such a lens, C′B′, in fig. 4, would represent the position of the photographic plate; and it is evident that, for all questions of illumination or enlargement, N_1F, or the distance from the plate to the point from which all the light may be supposed to emanate, should be introduced into the calculations, and will give the true results, or, at all events, more nearly the truth than if N_1F_2, the true principal focal length, had been used in its place. Thus, by recording the length N_1F in the Certificate of Examination, we always give more nearly what the photographer practically wants than if the length N_1F_2, or the true principal focal length, had been ascertained. But in any case the point raised in this paragraph could, if thought desirable, be met by focussing the plate in the centre of the field when the observation for focal length is made.

The second point raised, as to the theoretical correctness of the principal focal length as found at Kew, is with regard to the distortion of the image, which may be described as the results due to the theory of the nodal points being not strictly applicable except near

FIG. 5.



the centre of the field. In fig. 5, let N_1 and N_2 be the nodal points, F the principal focus, and B″ and C″ the images of the infinitely distant points B and C; if there is distortion, the lines SB″ and SC″, drawn parallel to the incident rays, do not cut the axis at N_1, the nodal point of emergence; let these lines cut each other at S, which may be called the principal point of similitude with regard to the images B″ and C″. This construction represents the Kew method of observation, and therefore SF is the distance found as the principal

focal length, thus introducing an error equal to SN_1 in the result; the focal length given is, in fact, the distance from the principal focus to the principal centre of similitude for the part of the plate where the observation is made. But here again, since BN_2C, the cone of incident rays, is spread over a disc on the plate of which $B''C''$ is the diameter (and not $B'C'$), the mean intensity of illumination of the plate between these points will vary inversely as $(SF)^2$; and, if the plate covers an angle larger than BN_2C, the C.I. numbering of the stops will give a better indication of the relative exposure on the assumption that SF is the principal focal length than if the true value N_1F is introduced into the calculations. Thus, what has been given in the Certificate of Examination will again be nearer what is practically required by the photographer than if the true principal focal length has been recorded. If, however, the lens is intended to be used for enlargements or reductions, and the final adjustment of the distance of the object is to be made by reference to tables, then, no doubt, the true principal focal length must be accurately given; but no photographer would ever use a lens showing sensible distortion within 15° of the axis, for such purposes, for, if he did, the ratio of the enlargement or reduction would vary sensibly in different parts of his plate; and, if there is no distortion within this distance from the axis, S and N_1 will be coincident, and the Kew method will give accurately and truly N_1F as the principal focal length. Thus, in the only circumstances under which the principal focal length is practically wanted with theoretical truth and great accuracy, it is seen that the results given in the Kew certificate do answer these requirements.

The third condition that has been laid down as being necessary before the Kew method gives theoretically correct results is that the nodal point should be the same for white light as for photographically actinic rays. This may be hypercritical, but if, in fig. 5, C'' and B'' represent the images as seen on the photographic plate, C' and B' those seen by the eye on the ground glass, N_1 the mean position of the nodal point of emergence for visible rays, and S the mean position for actinic rays, then it is evident that FN_1 will be the principal focal length found by the observation, whereas SF will be the quantity required in calculations with regard to enlargements or illumination. If the lens gives any distortion, N_1 would represent the centre of similitude for visible rays and S that for photographically actinic rays; the condition might, therefore, have been more rigidly defined by stating that the point of similitude for visible rays and that for actinic rays must occupy identical positions for parts of the field between the points of observation. As far as can be judged, this is a negligible source of error in all cases.

A fairly large angular movement of the swinging beam, about

$14\frac{1}{2}°$ on each side of the axis, has been adopted at Kew in order that any error in the measurements on the ground glass may produce a small proportional error in the results. But it should be observed that, the smaller this angle, the less will be the errors just discussed, and by lessening the angular movement these errors can be reduced to any extent, but only with a proportional loss in the general accuracy of the results obtained.

This is not the place to enter into a general discussion on focometry, but a few words to justify the choice of the Kew method may perhaps be permitted. Many of the known means of finding the principal focal length depend in principle on measuring the relative size of the object and the image, and the foregoing remarks on the errors involved are more or less applicable to them, thus showing that they are open to the same criticisms on theoretical grounds as the work at Kew. Many methods of focometry have to be rejected because they do not measure the distance from the nodal point, and others are unsuitable because the calculations or successive adjustments involved, render the operation too lengthy. There are no doubt many instruments, as for instance that devised by Professor Silvanus Thompson, which do give the true focal length as measured on the axis with theoretical accuracy, but these have not, as a rule, been specially designed for photographic lenses. One method, which is hardly open to criticism on theoretical grounds, may be mentioned in a little greater detail as being that specially recommended by the International Congress of Paris; this is the elegant plan which Commandant Moëssard proposes to carry out by means of his instrument, called the *Tourniquet*, which is described in Wallon's 'Traité élémentaire de l'Objectif Photographique,' and elsewhere. Advantage is taken of the principle that if a lens is revolved about an axis passing through the nodal point of emergence, the image of a distant point will not appear to move if seen through a fixed eyepiece; thus, by successive adjustments and trials, the lens can be so placed that an axis does pass through the nodal point; and, by measuring the distance between this axis and the focus of the eyepiece, the true focal length can therefore be obtained. Since a movement can be detected before it can be measured, a smaller angular movement is required with this method than with the Kew testing camera, and therefore, as far as distortion is concerned, greater, but not absolute, theoretical accuracy is obtained. As for the coincidence of the visual and actinic centres of similitude, better theoretical results are only obtained by this method on the assumption, which is probably a true one, that these points approach each other as the point of observation gets nearer the axis.

By taking observations some 14° away from the axis of the lens, we conclude, therefore, that we obtain the most rapid and

accurate method of focometry, and, in the case of the image within this limit being distorted, that the focal length thus obtained, even though it is not identical with the principal focal length measured on the axis, is what the photographer in reality wants to ascertain. The Kew method is, therefore, we believe, open to no criticism on theoretical grounds as far as the value of the results is concerned.

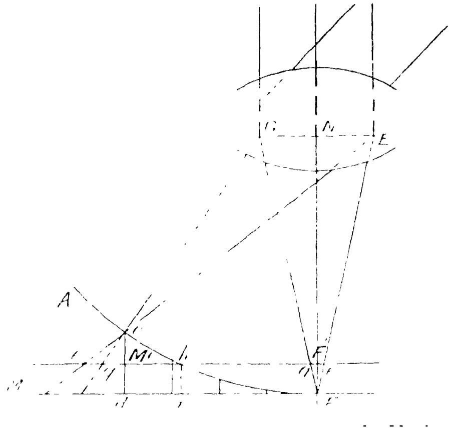
Under the same heading as the principal focal length is also recorded the "back focus," or the length, from the ground glass surface to the nearest summit of the lenses, when the focus is adjusted on a distant object. The difference between the principal focal length and the back focus therefore gives the distance of the nodal point of emergence from the inner summit of the lenses, thus enabling anyone to mark the place where the principal plane cuts the mounting. In symmetrical lenses, which are generally used for plan work, the position of the principal plane of incidence, or the point from which the distance of the object must be measured when regulating enlargements, can also be marked; for it then occupies the same relative position with regard to the furthest summit of the lenses (that is, to the outside end of the lens) as the nodal point of emergence does to the inner summit of the lenses.

12. *Curvature of the Field, or of the Principal Focal Surface. After focussing the plate as its centre, movement necessary to bring it into focus for an image —— inches from its centre = —— inches.*
Ditto for an object —— inches from its centre = —— inches.
" " —— " " = —— "

The following is the method of finding the curvature of the principal focal surface. The image of a distant object (or of the collimating telescope) is thrown on that point on the ground glass where the axis of the lens cuts it, the focus is accurately adjusted, and the focus scale is read off. The swinging beam is then moved so that the image comes successively to positions at convenient intervals from the centre of the plate, and on each occasion the focus is adjusted afresh, and the focus scale read off. By subtracting the central reading from these outer readings, the results recorded in the Certificate of Examination are obtained.

But a mere observation of the curvature of the focal surface does not at once indicate how serious is the evil effect of this defect in the lens. Further consideration is necessary to settle this point. If the results furnished by this test are plotted in the form of a curve, they will represent a section through the principal focal surface; let AF in fig. 6 be such a curve, and let cd $(=\mu)$ be the movement necessary to bring the plate into focus at its margin; let N_1F be the principal focal length, and $EG (=\epsilon)$ the effective aperture of the

FIG. 6.



lens. The effect of this curvature is to make the image of a point appear on the plate as a disc, except on the circle or at the point where the principal focal surface either cuts or touches the plate. If the photographic plate is in the position M′F′, such that it bisects *cd*, then the discs of diffusion will be greatest at the centre and at the margin of the plate; and any movement of the plate from this position will increase the size of the disc at one or other of these places; if the photographer adjusts his focus so as to produce the best general focus, *eg* ($= \delta$) will therefore be the diameter of the largest disc of diffusion on his plate. Assuming that this position of the plate has been adopted, and that the lens gives no distortion, then, by similar triangles, it can be seen that:—

$$\mu = \frac{2\,\delta}{\epsilon}(f-\mu) = \frac{2\,\delta f}{\epsilon} \text{ nearly} \qquad (1).$$

But the C.I. No. of stop $= \dfrac{f_2}{100\,\epsilon^2}$; and therefore

$$\mu = 20\,\delta\sqrt{(\text{C.I. No. of stop})} \qquad (2).$$

The following table gives the value of δ for different values of μ, and for stops of different numbers; and thus the size of the greatest disc of diffusion can at once be seen from the results of the examination as recorded in the certificate.

Table giving μ (the difference of focus in inches of the centre and the margin of the plate; or, after focussing the plate at its centre, the amount it has to be moved to bring the margin into focus) with reference to the size of disc of diffusion and number of stop.

C.I. No. of stop.	δ, the diameter, in decimals of an inch, of the maximum disc of diffusion when the plate is in the position giving the best general focus.									Approximate ratio of effective aperture to focal length.
	0·002.	0·004.	0·006.	0·008.	0·010.	0·012.	0·014.	0·016.	0·018.	
64	0·32	0·64	0·96	1·28	1·60	1·92	2·24	2·56	2·88	*f*/80
60	0·31	0·62	0·93	1·24	1·55	1·86	2·17	2·48	2·79	*f*/77
56	0·30	0·60	0·90	1·20	1·50	1·80	2·09	2·39	2·69	*f*/75
50	0·28	0·57	0·85	1·13	1·41	1·70	1·98	2·26	2·55	*f*/71
48	0·28	0·55	0·83	1·11	1·39	1·66	1·94	2·22	2·49	*f*/69
40	0·25	0·51	0·76	1·01	1·27	1·52	1·77	2·03	2·28	*f*/63
32	0·23	0·45	0·68	0·91	1·13	1·36	1·58	1·81	2·04	*f*/57
30	0·22	0·44	0·66	0·88	1·10	1·32	1·53	1·75	1·97	*f*/55
24	0·20	0·39	0·59	0·78	0·98	1·18	1·37	1·57	1·76	*f*/49
20	0·18	0·36	0·54	0·72	0·89	1·07	1·25	1·43	1·61	*f*/45
16	0·16	0·32	0·48	0·64	0·80	0·96	1·12	1·28	1·44	*f*/40
15	0·15	0·31	0·46	0·62	0·77	0·93	1·08	1·24	1·39	*f*/39
12	0·14	0·28	0·42	0·55	0·70	0·83	0·97	1·11	1·25	*f*/35
10	0·13	0·25	0·38	0·51	0·63	0·76	0·88	1·01	1·14	*f*/32
8	0·11	0·23	0·34	0·45	0·57	0·68	0·79	0·91	1·02	*f*/28
6	0·10	0·20	0·29	0·39	0·49	0·59	0·69	0·78	0·88	*f*/24
5	0·09	0·18	0·27	0·36	0·45	0·54	0·63	0·72	0·81	*f*/22
4	0·08	0·16	0·24	0·32	0·40	0·48	0·56	0·64	0·72	*f*/20
3	0·07	0·14	0·21	0·28	0·35	0·42	0·48	0·55	0·62	*f*/17
2	0·06	0·11	0·17	0·23	0·28	0·34	0·39	0·45	0·51	*f*/14
1	0·04	0·08	0·12	0·16	0·20	0·24	0·28	0·32	0·36	*f*/10
$\frac{3}{4}$	0·03	0·07	0·10	0·14	0·17	0·21	0·24	0·28	0·31	*f*/8·7
$\frac{2}{3}$	0·03	0·07	0·10	0·14	0·16	0·20	0·23	0·26	0·29	*f*/8·2
$\frac{1}{2}$	0·03	0·06	0·08	0·11	0·14	0·17	0·20	0·23	0·25	*f*/7·1
$\frac{2}{5}$	0·03	0·05	0·08	0·10	0·13	0·15	0·18	0·20	0·23	*f*/6·3
$\frac{1}{3}$	0·02	0·05	0·07	0·09	0·12	0·14	0·16	0·18	0·21	*f*/5·8
$\frac{2}{7}$	0·02	0·04	0·06	0·09	0·11	0·13	0·15	0·17	0·19	*f*/5·3
$\frac{1}{4}$	0·02	0·04	0·06	0·08	0·10	0·12	0·14	0·16	0·18	*f*/5·0

When judging the quality of a lens by means of the results given in this test, the above table may also be used in the following manner:—Decide on the value of δ (the diameter of the greatest disc that will be tolerated in the image of a point), and find, from the results recorded in the Certificate of Examination, the difference of focus, μ, between the centre and the extreme corner of plate; then, knowing these two quantities, the table at once shows what is the C.I. number of the stop that can be employed under these conditions, or, in other words, with what rapidity the lens will work.

It may also be remarked that this table gives for any part of the plate, and for stops of given size, the *radius* (δ) of the image of a point after the plate has been removed a distance, μ, from its proper focus in either direction, the movement being measured in a direction perpendicular to the plane of the plate.

According to the recommendations of the International Congress, lenses should generally be supplied with stops, numbered according to the proposed system, in the following series: 1, 2, 4, 8, 16, 32, 64, &c. I should have thought that the series, 1, 2, 3, 5, 10, 20, 30, 60, &c., would have been more convenient for the purposes of mental arithmetic; for example, with the two last stops in this series, the exposure would be the same multiple of the half minute or minute that the unit of exposure is of the second. Both series have therefore been included in the above table.

The results recorded in the certificate under this heading may possibly also be useful to the photographer in another way, by enabling him to decide approximately what part of the ground glass he should use when focussing. In fig. 6, let k be the point where the plate M′F′ cuts the principal focal surface when in the position which has been proved to give the best general focus; hence the image will be perfectly sharp at k, and conversely, if the focus is adjusted by looking at the point k on the ground glass, the plate will be brought to the position M′F′ required; but since kl is half cd, there is no difficulty by interpolation or plotting to find the approximate position of k for any given distance of the point c from the axis. Look in the Certificate of Examination for cd, the difference of focus between the centre of the plate and its margin, find in the above manner the position of k, where the difference of focus from the centre is only half cd, and we get the point on the ground glass which should always be used when focussing with all stops, if it is desired to get the best general focus.

13. *Definition at the Centre with the Largest Stop,* —— *C.I. Stop, No.* —— *gives* —— *definition over the whole of a* —— *inch by* —— *inch plate.*

The system by which the defining power is measured consists in ascertaining what is the thinnest black line of which the image is just visible, the test being conducted in the following manner. The test object consists of a thin straight strip of steel, about 0·1 inch wide, and about an inch long; it is capable of being rotated about an axis in the direction of its greatest length, thus, if seen against a bright background, making it appear as a black line of varying width; when presented edgewise to the objective, it is so thin that the image becomes invisible; and there is an arc so graduated that the angle subtended by the two edges of the strip at the lens can be at once read off, thus giving a measure of the apparent thickness of the line. The test-object is placed as far as possible from the lens in a darkened room (at Kew the accommodation in this respect leaves much to be desired), and beyond it is a ground glass screen illuminated by a lamp.

In order to test the defining power of a lens in the centre of its field, the focus is first very carefully adjusted on the ground glass, and the test-object is then slowly revolved from the edgewise position, where its image is invisible, until the first appearance of a dark line can be seen against the bright background; the angular width of the line is read off, and is noted as a measure of the defining power of the lens in the centre of its field. The light of the lamp is regulated so that the image of the line can be seen as soon as possible.

Besides measuring the defining power where the axis of the lens cuts the focal surface, an observation is also made at a point representing the extreme corner of the plate of the size for which the lens is being examined, that is, at a distance from the centre equal to half the diagonal of the plate. As the object of this second test is to measure the general definition over the whole plate, the focus is taken at a position half-way between the point of observation and the axis of the lens, this being the method generally adopted by practical photographers when desirous of getting the best general focus. It is necessary, moreover, that the test-object should be so arranged that the steel strip makes an angle of 45° with the horizon; for, since the diffusion of the image near the margin may be due to astigmatism, a false impression of the defining power will be obtained if the image of the dark line coincides in direction with either of the focal lines; whereas if it bisects the angle between them, as will then be the case, there is no error in the result from this cause. The test is not, however, conducted in quite the same way as in the first instance; the test-object is set at a known angle, and the stops are slipped in one

after another, beginning with the largest and going on to smaller ones, until the image of the black line on the bright ground is first just visible; the C.I. No. of the stop with which the lens gives definition up to a known standard at the extreme corner of the plate is thus ascertained, and, as it may fairly be assumed that the definition will be no worse than this at any other part of the plate, it follows that the defining power over the whole plate comes up to or exceeds the standard selected.

It cannot be denied that the defining power is the most important quality of a photographic lens for almost every purpose, and yet the best method of testing definition has never been satisfactorily discussed or considered. If a thoroughly good test could be devised, it would be hardly necessary to examine at Kew for curvature of field or for astigmatism, for these defects are only hurtful in so far as they affect definition. But it must be confessed that the method above described is open to some objections, and the following discussion is merely intended to show that it is the best that could at present be devised.

In considering this question, it was natural that attention should first be turned to the excellent arrangements adopted at Kew for testing the definition of telescopes. The method generally used, especially when dealing with instruments supplied for the public service, is to compare each one separately with a standard telescope by an observation on a distant object; telescopes sent for examination can by this means be *passed* or *rejected*, but hardly classified. But in examining photographic lenses, where there is a much greater variety of form and pattern, it would be quite out of the question to keep a sufficient number of standard lenses to be of any practical use. Thus little assistance was obtained from the experiences gained in the examination of telescopes.

It was necessary therefore to seek some method which did not depend on comparisons with standards, and in devising such a test the object most to be kept in view was evidently to diminish as far as possible the errors due to the variations either in the transparency of the atmosphere or in the personal qualities of the observer.

With regard to the first point, that is, the effect of fog, mist, and dust in the air, the only way to avoid errors from these causes appeared to be to conduct this test in a room. This was considered especially necessary in a climate like that of London. It is no doubt theoretically right to examine portrait lenses, or lenses for copying plans, by observations on a test-object not too far away; but for landscape lenses a distant test-object would, from other points of view, be preferable, and the adoption of the examination in a room was only the choice of the lesser of two evils.

With regard to variations due to the personality of the observer, the

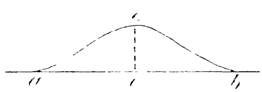
case is more difficult. Probably the most important consideration is that the test should not be based on a mere judgment, the reason for which one person cannot readily communicate to another. In many works on photography the extent of field over which the lens produces a "sharp" image is discussed, as if by mere inspection this could be determined; whereas no two people would exactly agree as to where the diffusion of the image was sufficient to be classed as want of sharpness, and no two objects would serve equally well for such a test. It is essential, at such an establishment as the Kew Observatory, that the observer should obtain some definite numerical result from his examination, even though it may be considered advisable to merely employ general expressions in the wording of the certificate; under any other system it would be impossible for any length of time to prevent the standards from varying.

Still more difficult is it to avoid errors from actual variations in eyesight, whether between different individuals or at different times in the same individual. Some general conditions may, however, be laid down. When the illumination of an object is very feeble, the subjective light of the eye, as it has been called by Helmholtz, plays an important part in determining the least intensity of illumination which is visible, and this subjective light is a very variable quantity; the eye increases in sensitiveness for a long time when light is excluded from it, the increase at first being very rapid, which may be another way of expressing the same fact. Hence, any feebly illuminated object must be a bad test-object, for its appearance will vary very materially according to the state of the eye. On the other hand, if the illumination is too bright, the eye will be much influenced by irradiation, and the subjective effect on the eye will be a bad indication of the true condition of the object; moreover, as irradiation is the effect on the appearance of an object produced by brighter surrounding objects, and as this effect diminishes as the differences of shade get less, the test-object should show no marked contrasts in illumination. But in applying these general remarks to the case under consideration, it must be remembered that it is not the test-object which is seen by the eye; it is the image of the test-object, as produced by the lens under examination. Hence, it appears that the test-object should produce an image of medium intensity of illumination, and one in which there are no great differences in shade. The test-object used at Kew, it will be remembered, consists of a perfectly black object seen against a bright background, and it might therefore appear as if it were not a good selection. In order to prove that, as a rule, the differences of shade in the image are small, and that no objections can be raised to the Kew test on theoretical grounds, it is necessary to show what is the effect on the image produced by a want of defining power in the lens.

The result of bad definition in the lens is to make the image of a point occupy a sensible area on the photographic plate, and consequently to prevent the image of the edge of a surface from being sharply indicated. The general effect can be best illustrated by means of figs. 7 and 8, where the abscissæ are enlarged dimensions measured on the plate, and the ordinates indicate the intensity of illumination at each point. In fig. 7 let *a*, *e*, *b* represent a section through the image of a small spot of light. In fig. 8 let the curve *f*, *h*, *k* represent the actual image of the edge of a bright surface, which would be represented by *f*, *d*, *c′*, *k* if the defining power of the lens were perfect; it is evident that *a′ b′* in fig. 8 is equal to the limiting value of *ab* in fig. 7 as the spot of light becomes infinitely

FIG. 8.

FIG. 7.



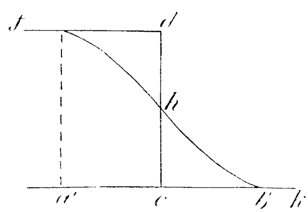
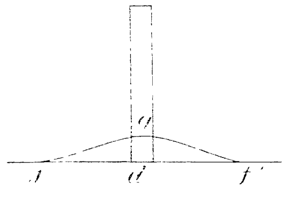
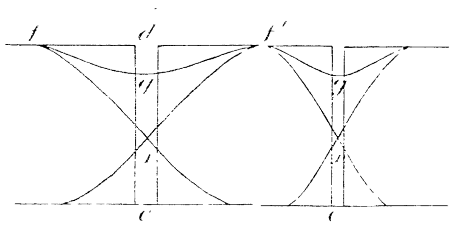


FIG. 9.



FIGS. 10 AND 11.



small. In fig. 10 is shown the effect of bringing two bright surfaces near together; that is to say, of a dark line as seen against a bright background; f, g, f' will represent a section through the image of the line, gx being equal to xc. If this curve is turned upside down, as in fig. 9, it can be shown that it represents the image of a bright line on a dark ground.

In this latter case—that of the bright line on the dark ground—it can be readily seen that the effect of narrowing the slit of light will be to decrease the illumination gd at the centre of the line until it becomes zero as the slit closes. The worse the definition of the lens, the sooner will the centre of the line reach the limit of visibility; but by ascertaining what is the width of the finest bright line just visible, a good test for defining power will not be obtained for the following reasons; in the first place, the illumination of the image will be feeble, which has already been shown to be objectionable, and in the second place, since with feeble illuminations the ocular sensation varies as a first approximation as the intensity of the illumination, considerable errors would arise through the difficulty of obtaining a constant illumination through lenses of different types.

These objections do not apply, however, to testing definition by finding the width of the finest dark line that can be seen against a bright background. In this case as the line becomes thinner, the illumination at its centre increases until it reaches that intensity of illumination which can no longer be distinguished by the eye from the illumination of the field. If the illumination cg in fig. 10 can be distinguished from cd by the eye, it is evident that a blurred image of the dark line is visible, and if any illumination greater than cg is indistinguishable from cd by the eye, it is evident that the figure represents the image of the thinnest black line which is visible. Fig. 11 represents generally the same condition of things as that shown in fig. 10, except that the defining power of lens is much better; and it will be seen how much finer the line must be in this case to produce the same proportional illumination at its centre; that is to say, before the limit of visibility is reached. Now there is a certain intensity of illumination at which and about which the eye is at its maximum of sensitiveness to differences of shade, and this is when the object is what would be described as not bright and not dark; between these wide limits the minimum difference of shade visible is a fixed proportional part of the total illumination. This proportion differs with different observers, but not to a very great extent. Hence if a plan is adopted by which a dark line on a bright ground can be made to vary in thickness, and if the illumination is arranged so that the eye is at its maximum sensitiveness (that is therefore so that the line remains longest visible as it diminishes in width), then the moment at which it disappears will occur when the

difference of intensity of illumination of the centre of the line and the field is the minimum difference of shade discernible by the eye, and this will be independent of the actual intensity of the field, and will not vary much with different observers. But it has been shown that the thickness of the line does vary with the defining power of the lens, and it may therefore be concluded that the test adopted at Kew is not open to serious objections on theoretical grounds.

In the foregoing discussion, it has, however, been assumed that the curve representing the image of the edge of a surface is such as that which Helmholtz has shown to be produced as an ocular effect by the circles of diffusion being due to want of accommodation of the eye itself:* it will be observed that no part of the curve is tangential to the vertical. If, however, the curve is similar to that given by the

FIG. 12.

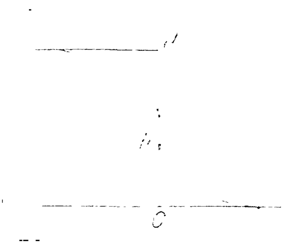
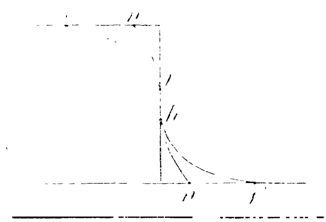


FIG. 13.



same author as being due to dispersion in the eye, and illustrated in fig. 12, it will be seen that the result of gradually diminishing the thickness of a line will not be exactly as above described; for, however thin the dark line on the bright ground becomes, the intensity of illumination at its centre can never be more than twice *ch*; and if the ratio of twice *ch* to *cd* is less than a given ratio, the image of the black line will remain visible until it is so thin that the eye cannot perceive it. Therefore it might come about that two lenses giving images of the edges of surfaces as different as *flhf′* and *nlhn′*, as shown in fig. 13, might give equally good results under the Kew test for definition, because in both cases the limit of visibility would be due to the minimum size of the line visible by the eye, and would have nothing to do with the definition of the lens. Helmholtz remarks on the very little evil effect of a diffusion represented by the curve shown in fig. 12, since the true edge is always visible. Hence we may assume that the Kew method still gives in such cases a good practical test for definition, though it does not test the amount of dispersed light over the image of fine lines, or, as a photographer

* 'Optique Physiologique,' Helmholtz, Paris, 1867, p. 185.

would say, the brilliancy of small objects. In fact, since the definition of an objective could only be rigorously expressed by a curve (or, more accurately, a surface) with dimensions, it is impossible for any one result to give all the information on this head which might be desirable.

As the eye is capable of detecting a difference of shade of about 1 per cent. of a moderately illuminated field, it will be only necessary for the curve shown in fig. 12 to be tangential to the vertical for 1 per cent. of its height to render the image of an infinitely thin line visible in so far as that visibility depends on difference of shade. But take the case of a line not absolutely black, and seen against a bright background; then, in fig. 10, the illumination of the centre of the image will be represented by *gc*, *plus* some proportional part of *gd*; in comparison with the case of the absolutely black line, it can be shown that the curve must be tangential to the vertical for a proportionately greater distance before the shade of the centre of the image of the infinitely thin darker line will be sufficiently deep to form a visible contrast. For instance, if the line is illuminated to nine-tenths of the intensity of illumination of the field, the curve must be tangential to one-tenth of *dc* (see fig. 10) before this condition of things occurs. A test depending on the thickness of a line which is darkened to a definite proportional intensity of the field would therefore present this disadvantage, that there would be fewer occasions on which different degrees of imperfection of definition of lenses would show the same result in testing; such a test may therefore in future be adopted at Kew.

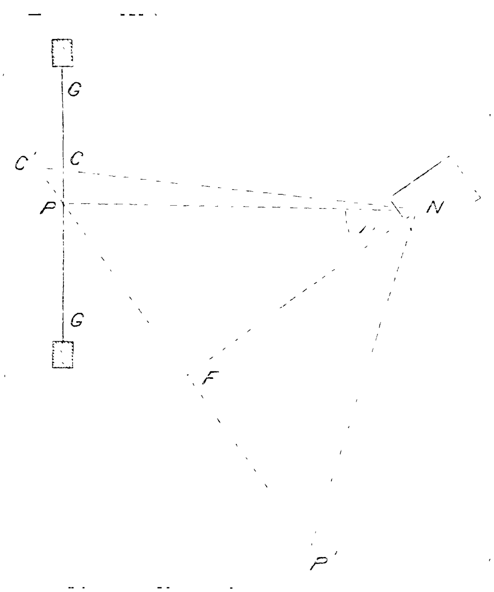
It should, however, be remarked that in the whole of the above reasoning it has been assumed that the minimum proportional difference of shade visible is the same in a thin line as in a thick one, which can hardly be the case. But this false assumption will not, it is thought, vitiate the general conclusions arrived at.

It is of course conceivable that the actinic rays will be brought to either a better or to a worse focus than the visible rays; it is believed, however, that no serious error is likely to result from the test being done by the eye and not by photographic methods; it is almost certain that the curve representing the edge of a surface will have the same general character in the two cases, and therefore that the results obtained with the eye will be a good indication of those which would be obtained by photography.

14. *Distortion. Deflection or sag in the image of a straight line which, if there were no distortion, would run from corner to corner along the longest side of a —— by —— plate = 0·—— inch.*

The following is the method adopted at Kew of measuring the distortion produced in the image by the lens under examination. Let fig. 14 be a vertical section through the testing camera; GG re-

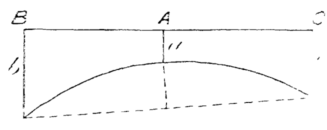
FIG. 14.



presenting the ground glass; F the principal focus; and N_1 the horizontal axis, which passes through the nodal point of emergence, the adjustment for that purpose having already been made for test No. 10. The lens-holder carrying the lens is first turned in either direction through an angle β, such that PF, or $FN_1 \tan \beta$, or $f \tan \beta$ is equal to half the *shortest* side of the plate for which the lens is being tested. (The *horizontal* movement of the swinging beam in the testing camera gives an easy means of determining the angle β; a distant object is first brought to focus at the centre of the ground glass, and then the swinging beam is revolved about the axis A (see fig. No. 1) until the image has moved along the graduated scale a distance equal to half the shortest side of the plate; the beam is thus made to move through the angle β, which can be read off with sufficient accuracy on BC, the top of the wooden stool, which is graduated for that purpose). After this adjustment has been made,

the ground glass is brought into focus by observing the image of a distant object at a point P, a little below C, the line engraved on the glass; under these circumstances, if the principal focal surface is a plane, and if the lens were being used in the ordinary manner, PP′ would be the position occupied by the photographic plate, the section shown being taken across the centre of the plate parallel to its shortest side. The small distance PC is carefully measured; this length is then multiplied by secant β, thus obtaining C′P, which we will call a. The swinging beam is now revolved about the pivot in either direction, so that the image moves along the scale on the ground glass a distance equal to half the *longest* side of the plate for which the lens is being examined; the sketch in fig. 7 is still more or less applicable, PP′ still representing a section across where the photographic plate ought to be, but this time at the end of the plate, not at its centre; (F, therefore, no longer represents the principal focus); in fact, what has been done is to make the image describe what, neglecting distortion, would be a straight line from the centre to the corner along the longest edge of the plate: after this movement has been made, the length of C′P is again obtained by measurement and calculation, and this time let the result be called b; the operation is repeated when the swinging beam is revolved to an equal angle on the other side of zero, and a third length, c, is thus obtained. In fig. 15, let BAC be equal in length to the longest side of the plate,

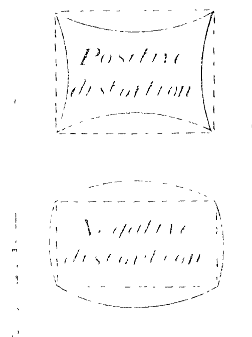
Fig. 15.



and let a, b, and c be the lengths just obtained; then the curve bac will evidently represent the image of a straight line thrown by the lens under examination along the edge of the longest side of the plate. Since the image travels along a line very nearly parallel to the engraved line on the ground glass, BAC will be nearly parallel to the chord of the curve, and $\frac{b+c}{2}-a$, which is the length recorded in the Kew certificate, will be a very close approximation to the sagitta or sag of the curve.

The image of a rectangle near the limits of a photographic plate will appear, when any distortion is visible, like one or other of the forms indicated in fig. 16. The sagitta is conventionally considered

FIG. 16.



positive if it is measured towards the centre of the plate from the chord, thus giving the name of positive and negative distortion in the two cases.

The distortion for distant objects is not necessarily exactly the same as for nearer ones, and therefore the uses for which the lens is intended should not be forgotten; for example, with portrait lenses, an object some 10 to 20 ft. away should be used to throw the image in the above test.

Probably it will not at once be admitted that this is the best means of measuring distortion; for no doubt it might be done in many other ways, and a method might easily have been selected which would have been less open to criticism on purely scientific grounds. We believe, however, the Kew certificate gives the information really required in practice. In order to determine if a lens is suitable for any particular purpose, all that is required to be known is whether the image of a straight line near the edge of the plate will show too much curvature, the amount of tolerance depending greatly on the work for which the lens is to be used. There is no means of enabling the photographer to form a judgment on this point more readily than by giving him the sagitta or sag in the image of a straight line along the edge of his plate. That it would be difficult to find a better method may, perhaps, be made more evident with the aid of figs. 17 and 18, the former representing a section through a lens and the photographic plate, and the latter showing part of the plate in plan, with the curved image of a straight line just inside its margin. In fig. 17, let N_1 be the nodal point of emergence; S_β the centre of similitude for rays emanating from a distant object and making an angle β with the axis; and S_θ the same for an object at an angle θ; e and g will, therefore, be the images of these two objects as seen on the plate, whereas, if there had been no distortion, they would have appeared

Fig. 17.

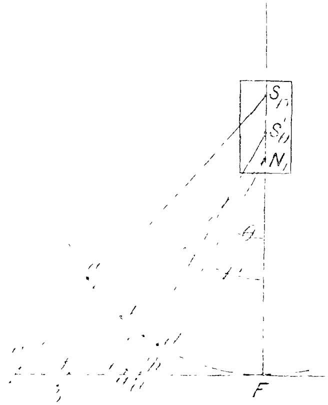
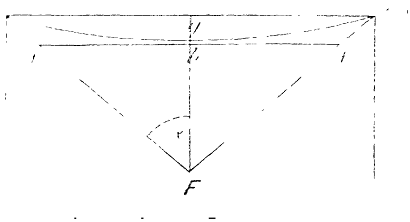


Fig. 18.



at f and h respectively; ef and gh will, therefore, be the total distortion in each case. In fig. 18, let the rays coming from the objects, of which the images are seen at e and g, make the angles β and θ with the axis of the lens at the nodal point; if ef and eg are equal in length to the lines similarly denoted in fig. 17, it is evident that the curve ege represents the image of a straight line, which, if there had been no distortion, would have appeared as the line fhf. Now it would not have been difficult to have devised means of measuring the total distortion at any part of the plate; for instance, to have measured the distortion ef for the point e at the corner of the plate—but the following considerations show, it is thought, that that would not be a suitable way of testing the lens; let the curve efe in fig. 18 represent the greatest curvature that would be tolerated for the class of work for which the lens is intended; compare the lens producing this curve with another in which S_β occupies the same position, but in which S_θ is nearer the

nodal point N_1; ef would be the same in the two cases, but gh would be less in the second case, and the curvature would therefore exceed the tolerated limit: with two lenses giving an equal total distortion at the margin, one should be passed and the other rejected. The total distortion at any one point will not therefore give a measure by which the lens should be judged, the greatest rate of change in the distortion more nearly representing what is required to be known; and, as the rate of change is certain to be greatest at the margin, the Kew certificate supplies the information required.

The tourniquet has already been mentioned as an apparatus which has been specially recommended for the purpose of testing photographic lenses; by means of this invention, Commandant Moëssard obtains an excellent means of detecting distortion, but hardly of measuring it in a way to indicate the curvature produced in an image. It will be remembered that the lens can be revolved about an axis which passes through the nodal point N_1, whilst the eyepiece remains stationary; the effect of this movement can be seen in fig. 17 by imagining the lens to be stationary, whilst the object and the eyepiece revolve about the nodal point, the arc $abcd$F being the path traversed by the eyepiece. Let a be the image of the object after the lens has been revolved through an angle β, and e the position where the image would be seen on the photographic plate; for there is no reason to believe that the line ea will coincide exactly with the line eS_β; if there were no distortion, b would be the image as seen in the tourniquet, and the distance moved by the image from b to a is what is measured by that apparatus. It will be noted that the image a will be much out of focus if the lens has a fairly flat field; and that, after re-focussing, a' will represent the image, and b' the point from which the measurement is taken; this re-focussing will tend to reduce any error which may be due to ae not being coincident with eS_β, but such a movement in the middle of an operation is rather objectionable on mechanical grounds. Putting this objection aside, it will be seen that we do not get a ready means of finding the curvature produced in the image as seen in plan in fig. 18; for, if $c'd'$ is the length measured by the tourniquet when the lens is revolved through an angle θ, then the sagitta of the curve is equal to

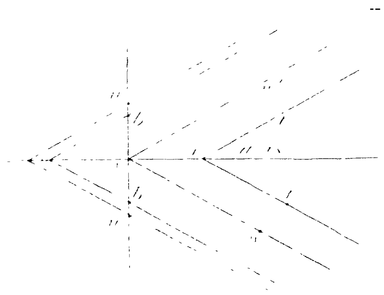
$$a'b' \sec \beta \cos \alpha - c'd' \sec \theta.$$

15. *Achromatism. After Focussing in the Centre of the Field in White Light, the Movement necessary to bring the Plate into Focus in Blue Light (dominant wave-length* 4420), = 0· —— *inch. Ditto in Red Light (dominant wave-length* 6250), = 0· —— *inch.*

The photographer may be said always to adjust his focus in daylight, and if the actinic rays are not brought to the same focus as the

dominant rays for white light, the definition obtained in the photograph itself cannot be perfect. In fig. 19, let $u'wu'$ be the position of

FIG. 19.



the photographic plate, the focus of which has just been adjusted in daylight; if the lens has not been properly corrected for achromatism, the different coloured rays will form different cones, and those coming to a focus at w will have a wave-length of about 5570, for that, I am informed by Captain Abney, is generally speaking the dominant wave-length for white light. Let $b'bb'$ be the cone of rays of 4420 wave-length, which is not far from the position of the maximum actinic effect for ordinary bromide dry plates, and let $u'uu'$ be the cone for rays of 4000 wave-length; since the actinic effect with silver salts begins to fall off rapidly at about 4000 wave-length, the cones outside the cone $u'uu'$ may be neglected, and it may be taken that the image of a point covers a disc on the photographic plate of which $u'w$ is the radius. It is evident that what the photographer wants to know, with regard to the achromatism of his lens, is the amount of diffusion caused in the image by any errors in its construction, that is to say, what is the actual size of the disc of diameter $u'w$.

The examination for achromatism is therefore made in the following manner:—First the focus is carefully adjusted in daylight on a suitable object placed as far away as possible in the room, and then the focus scale is read off. After this, a sheet of blue glass, the colour of which has a dominant wave-length of 4420, is placed behind the object and close in front of a small opening in the shutter through which all the light enters the room; the focus is re-adjusted, the focus scale read off again, and the difference in reading to that observed in white light is noted; the length bw in fig. 19 is thus obtained. Now let f be the principal focal length of the lens; and let f' be the focal distance when the observation was made, which can be easily obtained with sufficient accuracy by a direct measurement

from the ground glass to the nodal point of emergence, or to the pivot which has been made to pass through that point. The difference of focus, bw, noted between the blue and white light is then multiplied by f/f', and the result thus obtained is that finally recorded in the Certificate of Examination as if it were the direct result of an observation made on a distant object. Exactly the same process is then repeated with a sheet of red glass, the colour of which has a dominant wave-length of 6250.

The reason for multiplying the result of the observations by f/f' is that it would evidently be unfair to test objectives of different focal lengths on a near fixed object, for in some cases the ground glass would be close to the principal focus, and in others far removed from it. It seems, therefore, advisable to reduce all results so as to make them equivalent to observations taken on infinitely distant objects, and this is done by applying this correction. An assumption is here made that the difference of focus between different coloured rays in the same lens varies directly as the focal distance, and this in all probability, though not strictly accurate, introduces an exceedingly small error in the results.

The blue and red glasses, which were selected and measured for colour by Captain Abney, form a perfect contrast, as may readily be seen by placing them together and observing how very nearly completely all light is excluded.

By simply noting the difference of focus recorded in the certificate between observations made in red and white light, or between observations made in blue and white light (the latter being of far more practical importance), it can at once be told if the lens is or is not well corrected for achromatism. But it would seem desirable, as already remarked, to form an estimate of the actual amount of diffusion produced in the image as a result of any error that may be detected in the chromatic adjustment of the lens. Now there can be no difficulty in determining the size of the disc of radius $b'w$, for bw has been directly determined by experiment, and, since the cone $b'bb'$ represents the cone of rays of maximum actinic effect, on this disk will be concentrated the bulk of the rays which produce the effect on the photographic plate. But what we want in reality to find is the radius $u'w$, since that has been shown to represent more accurately the radius of the disc of diffusion; it may, however, be remarked that no fault can be found on this head with the method of testing, because the probabilities of error are lessened by taking the observation with rays of the maximum actinic effect. With a lens not at all corrected for achromatism the length between the different foci for different coloured rays varies approximately as the difference of the squares of the wave-lengths of the colours in question; and, taking the wave-lengths as above given, uw will be found to be to bw as 5 to

4. But it must be confessed that this rule may have little or no relation to the truth with a corrected lens, and it is merely adopted as the only approximation obtainable. It is assumed, therefore, that $uw = 5/4\,bw$. Let bw, the result obtained by the examination for achromatism, $= \alpha$; let the diameter of the disc of confusion, or twice $u'w$, $= \delta'$; let the principal focal length of the lens $= f$; and let the effective aperture $= \epsilon$. Then it can be seen, by reference to fig. 6, that—

$$\alpha = \tfrac{4}{5}\frac{f\delta'}{\epsilon} = 8\delta'\sqrt{(\text{C.I. No. of stop})}.$$

The table on p. 427, which gives the values of $20\,\delta\sqrt{(\text{C.I. No. of stop})}$, affords a ready means of obtaining the required results in the following manner:—

Knowing the C.I. No. of the stop, decide on δ', the diameter of the maximum disc of diffusion that will be tolerated; then, under the columns thus ascertained, look out μ in the table, multiply the figure there given by $\frac{2}{5}$, and the maximum difference of focus, α, that can be tolerated between white and blue rays is thus obtained.

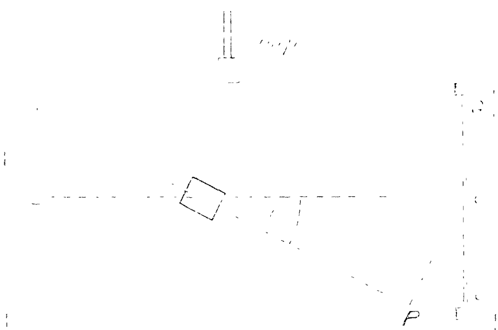
Or, in the line opposite the stop of the size under consideration, find a number equal to α, the observed difference of focus for white and blue rays; then δ', the diameter of the disc of diffusion, will be 5/2 times δ, the figure given at the top of the column in which α has been found.

It may be observed that the either the principal focal length or the position of the nodal point of emergence may vary as different coloured lights pass through a lens. It would not be difficult to investigate these two sources of error separately, but the results would be of little or no practical value.

16. *Astigmatism. Approximate Diameter of the Disc of Diffusion in the Image of a Point, with stop C.I. No.—— at —— inches from the centre of plate = 0· —— inch.*

The following is the method of examination for astigmatism:— The room is darkened, and in front of the lens is placed a thermometer bulb, thus obtaining, by means of the reflection of the light of a small lamp, a fine point of light. The lens holder of the testing camera is revolved upwards or downwards about the horizontal axis so that the axis of the lens makes an angle, ϕ, with the path of the rays coming from the thermometer bulb; the angle ϕ is such that the point of observation represents the extreme corner of the plate of the size of which the lens is being examined; that is to say, if, in fig. 20, GG represents the position of the ground glass, then CP is equal to half the diagonal of the plate; this angle has already been found for previous tests. If the lens shows any astigmatism, the image of

FIG. 20.

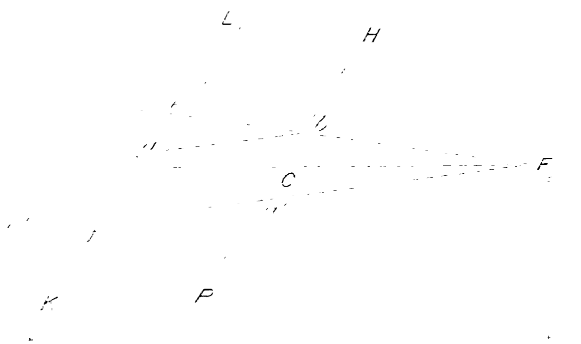


the point of light can be made to appear, first as a fine vertical line, and then, as the focus is lengthened, as a fine horizontal line. The focal scale is read off at each of these positions, and the difference, γ, between the two readings gives a measure of the astigmatism. But, in order to judge of the amount of astigmatism that can be tolerated, the diameter, σ, of the disc of diffusion caused thereby should be calculated. This is done by multiplying γ, the difference of focal distance of the focal lines, either by $\frac{f}{f'^2}\frac{\epsilon\cos\phi}{2}$ or by $\left(\frac{f}{f'}\right)^2\frac{\cos\phi}{20\sqrt{(\text{C.I. No. of stop})}}$, where ϵ is the effective aperture, f the principal focal length of the lens, and f' the focal distance when the observation was made. As the thermometer bulb is placed at the same distance from the testing camera as was the object in the examination for achromatism, the ratio f/f' is exactly the same as in that case. The same result may be obtained by the use of the table on p. 427 in the following manner:—Find the value of δ, the diameter of the disc of diffusion, on the supposition that the μ of the tables has the value just obtained for γ; multiply the value thus obtained for δ by $\left(\frac{f}{f'}\right)^2\cos\phi$, and we get σ, the required value of the disc of diffusion due to astigmatism. This is the quantity recorded in the Certificate of Examination.

That this is the case can readily be seen by reference to fig. 21. Here AB represents the effective aperture, F_1 and F_2 the positions of the focal lines, and PH the position that the photographic plate would occupy. At F_2 the image appears as a fine line perpendicular to the plane of the paper, and at F_1 it is represented by the line ab; half way between these two points the rays cut the plate in the form of a disc, of which $a'b'$ is a diameter. Any movement of the plate from this position must lengthen out the disc of diffusion in one

FIG. 21.

FIG. 22.



direction or the other, and this, therefore, is the position that the photographer naturally adopts as his focus. By similar triangles—

$$a'b'/AB = F_2C/F_2N_1 \text{ and } F_2C/CN_1 = F_2H/N_1P.$$

Therefore, since CN_1 and F_2N_1 are approximately equal,

$$a'b' = AB \,.\, F_2H/N_1P, = \frac{\epsilon}{2f'} \cos\phi \; F_1F_2.$$

Now F_1F_2 represents γ, the movement of the ground glass, which was the measurement recorded. In the case of the examination for achromatism, it was shown that it was unfair to conduct the test on a near

object without applying a correction, so as to make the result equivalent to an observation on a distant object, and that this correction could be made by multiplying the measurement recorded by f/f'. For the same reason, $a'b'$ must be multiplied by f/f' in this instance to obtain the true value of σ. Thus—

$$\sigma = \frac{f}{f'^2}\frac{\epsilon \cos\phi}{2}\gamma = \left(\frac{f^2}{f'}\right)\frac{\cos\phi}{20\sqrt{(\text{C. I. No. of stop})}}\gamma.$$

In considering the combined effect of astigmatism and curvature of the field, it should be remembered that it has been assumed that the photographer would focus his plate in the position PH, as shown in fig. 21, and that the principal focal surface, PH, was a plane; this is, however, never the case. If the focal surface is curved, it is evident that the best general focus is obtained by observing the image of an object at a position about half way between P and C on fig. 21. In fig. 22, which is part of fig. 21 enlarged, let KL be the position of the plate when focussed in this manner, the distance between KL and PH being, therefore, due to the curvature of the field. Through b' draw $b'g$ parallel to F_2a'; then, by comparing this figure with fig. 6, it will be seen that eg in both cases represents the diameter of the disc of diffusion due to the curvature of the field. Since, in fig. 22, fe represents the longest diameter of the ellipse of diffusion due to the combined effects of curvature and astigmatism, and since it is equal to the sum of $a'b'$ and ge, the diameters of discs of diffusion due to these two causes taken separately, it will not be unfair to look upon the evil effect of astigmatism as a simple addition to the evil effects of curvature. In using the table given on p. 427 in the manner described on p. 443, it would therefore be better if we subtracted the diameter of the disc of diffusion due to astigmatism from the diameter of the maximum disc of diffusion which is to be tolerated, and used the difference as the δ in the table; we should thus get a more correct notion of the size of the stop that could be used to obtain any required standard of definition. The objection to this use of the table is that the astigmatism, that is, the distance F_1F_2, varies to a certain extent with the size of the stop used during the observation.

In the above discussion it has been assumed that the focal lines are sharply defined. If this is not the case the reasoning here given is defective, because the distance separating the focal lines is then no indication of the amount of diffusion. An exaggerated idea of the amount of diffusion due to astigmatism may thus be obtained by the above method of calculation, for the disc may have only half the diameter thus found. Therefore, unless the focal lines are sharp—that is, unless the image of a point appears as a *very* thin line, first in one direction and then in another—no entry is made in the certificate.

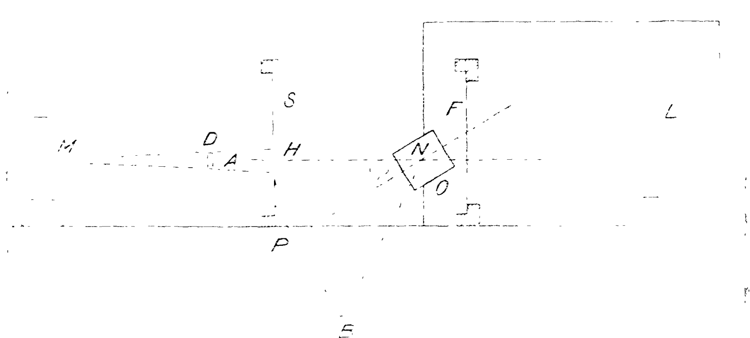
Objections have been raised to the use here made of the term *astigmatism*, when it is intended to mean the effect of spherical aberration on oblique rays; it has been proposed to limit the use of the word so as merely to signify cylindricity in the lenses, such as might be produced by turning them in a lathe with elliptical motion. Whatever may be the theoretical value of this objection, we fear that the use of the term has been so thoroughly incorporated into the photographic vocabulary, both in England and abroad, that it would now be impossible to substitute another expression in its place.

17. *Illumination of the Field. The figures indicate the relative intensity at different parts of the plate.*

With C.I. Stop No. .			With C.I. Stop No. .	
At the centre.............	100	:	Ditto..................	100
At inches from the centre		:	Ditto..................	
At inches from the centre		:	Ditto..................	

The intensity of illumination of the field is always greatest near the axis of the lens, and falls off more or less rapidly towards the edges of the plate. The lens should therefore be examined with the view of ascertaining if this inequality of illumination is greater than that which experience shows must be tolerated under given circumstances. The apparatus employed for conducting this test is shown in fig. 23, the method being devised by Captain Abney. There is a

FIG. 23.



fixed lamp, L, the position of which is not changed during the observations; F represents a paper screen, placed in that position in order to give a practically uniform source of light; O is the lens, which is fixed in a frame, not shown in the sketch, revolving about the pivot N; by means of a suitable adjustment, this axis, N, is made to pass through the nodal point of emergence of the lens. At S there

is a sheet of cardboard with a small hole in the centre at H, and this screen, hole and all, is covered with thin white paper on the side away from the lens; the distance between H and N is always made equal to the principal focal length of the lens; the bar D is made to cast a shadow from the movable lamp M on the paper just over the hole in the cardboard; thus, in this shadow, the paper is illuminated entirely by transmitted light from the lens, whilst the paper round it is illuminated entirely by the light of the movable lamp.

An observation is made in the following manner:—The lens is first placed in such a position that its axis passes through the hole H; the lamp M is then moved backwards or forwards until the transmitted illumination of the paper at H is made to match as nearly as possible the reflected illumination of the paper round it; the distance between S and M is then noted. The lens is now placed in the position shown in fig. 23, where AB represents the length of the diagonal of the plate for which the lens is being examined, and where the angle ϕ is half the angle of field under examination. The balance of light is readjusted by a movement of the lamp, and the distance MS is read off a second time. By finding the inverse ratio of the square of these two readings, we thus obtain the ratio between the illuminations at P and H, the lens being in the position shown in the sketch, and the object being supposed to be equally illuminated in both cases. But what is wanted is the ratio between the illuminations on the plate at P and A; this is found with perfect accuracy by multiplying the ratio of the illumination at P and H, as above obtained from the observations, by $\cos^3 \phi$, and this result is that which is entered in the Certificate of Examination. The relative illumination of the centre and of any part of the field can, of course, be obtained in this manner, in the above instance the corner of the plate being the point chosen.

This test may with advantage be made with the largest stop supplied, and also with the stop which has been shown, under test No. 13, to give good definition over the whole plate.

It cannot, however, be denied that there are objections to this method of examination. The fact that the illumination of the plate is not uniform is due to several causes:—(1.) The amount of light which passes through any aperture evidently diminishes with the obliquity. (2.) With lenses not free from distortion, the effective aperture itself varies with the angle of incidence. (3.) The amount of reflection from the surfaces of the lenses, and consequently the amount of transmitted light, varies with the angle of incidence. The method of observation above described may be said to fully take into account these three causes of variability in intensity. Then again (4) the light falling on the plate varies inversely as the square of its

distance from the nodal point, and also (5) with the obliquity with which the rays strike the plate. As far as these two latter considerations alone are concerned, it is evident, therefore, that the illumination on the plate varies as the third power of the angle of incidence, and also that by multiplying the result obtained on the screen at H by $\cos^3 \phi$ we obtain the required result on the plate at A. Thus the record in the certificate includes all these first five causes of irregularity of illumination. But there are other causes which are not correctly represented in this method of examination. In lenses not free from distortion the nodal point of emergence varies in position with the angle of incidence, and as the pivot N does not shift its position with reference to the objective during the observation, the condition of illumination of the photographic plate cannot be accurately represented. This is probably a trifling cause of inaccuracy; but one somewhat serious source of error remains to be mentioned. The method of examination does allow for (6) the variation of illumination due to the different amount of glass through which the oblique pencils have to travel; but, as the observation is made by eye, no allowance can be made for the fact (7) that the actinic rays may be affected in this manner out of all proportion to the apparent variations produced in the visible rays.

The method of examination adopted at Kew assumes that the light transmitted through the paper, as well as that reflected from the paper, varies in amount with the intensity of the incident light. Captain Abney informs me that his experiments prove this to be the case. But in making the observation the eye should be placed in the same position during both readings; for we have no reason to suppose the transmitted and reflected lights vary in the same way with the angle of vision.

It is impossible to suppose that the screen F will be illuminated with perfect regularity, even near its centre, and this must be a source of error, though probably a negligible one. When the axis of the lens passes through H the rays which are brought to a focus at that point, will be parallel to each other as they enter the lens; but when the axis of the lens is inclined this cannot be the case, for H will no longer be on the principal focal surface; the screen F should therefore be brought as near the lens as possible, as by that means the part of the screen from which the light comes will be more nearly identical in the two cases. The lamp L should, moreover, be placed as far from the screen as practicable, so as to make the illumination as uniform as possible. With lenses in which the nodal points are some distance apart, the part of the screen from which the light comes will vary considerably with the inclination of the axis, and considerable errors might be introduced by uneven illumination of the screen.

In deciding on the quality of a lens as regards the illumination of the field, this test should be considered in connexion with test No. 10, under which heading are given the angles of the cones of illumination. With regard to the normal use of any lens, except perhaps such as are specially designed for portraiture, certainly the whole of the smallest stop, and, as a rule, the whole of the largest normal stop, should be visible from the whole of the plate; for if the plate extends much beyond the limits of the inner cone (outside which the aperture begins to be eclipsed) the falling off of density near the edges of the plate will be a serious defect in the photograph. When considering the part of the field lying within this inner cone, it is to be noted that, the wider the angle which the lens covers, the greater is the inconvenience caused by the diminished density near the margin; if the stop is in front of or behind all the lenses, the intensity of illumination of different parts of the plate can be shown in this case to vary approximately as the fourth power of the cosine of the angular distance from the axis of the lens, and in cases where the stop is between two lenses, the limits of variation will be the third and fourth powers of the cosine of the angle. The following table is therefore inserted to give an approximate idea of the decrease of illumination as we recede from the axis of the objective, the truth lying *theoretically* somewhere between the two limits here given:—

ϕ.	$\text{Cos}^3\,\phi$.	$\text{Cos}^4\,\phi$.
0°	1·00	1·00
5	0·99	0·98
10	0·96	0·94
15	0·90	0·87
20	0·83	0·78
25	0·74	0·67
30	0·65	0·56
35	0·55	0·45
40	0·45	0·35
45	0·35	0·25

Eminent lens makers have spoken of the illumination produced by their lenses as being uniform from the centre to the margin, but our experience is that the decrease is even more rapid than here indicated. The above table shows how very objectionable is the use of wide angle lenses, whenever they can possibly be avoided. It shows, moreover, that the theoretical exposure for different stops should be materially modified according to the angle which the lens covers; for instance, taking the last column to represent the truth, it would be right, even though the stops in the two cases had the same C.I. number, to give half as much exposure again with a 90° objective as

with one only covering 40°, in order to get the same mean exposure over the whole plate.

In connexion with this test it may be mentioned that the most serious omission in the Kew examination is, that there is nothing to show the actinic transparency of the glass. A slight yellow tinge in the lenses, which would not be noticed by the eye, might yet be sufficient to seriously affect the rapidity of the objective. But no test could be devised to investigate this point which did not introduce photographic methods, and, as already stated, the consideration of expense put such operations out of consideration for the present. I should like, if possible, to have introduced some test which would have at the same time indicated the actual rapidity of the lens, and also the actual falling off of density towards the margin of the photograph; with the aid of photography this would not have been difficult, and a plan of this kind would have been adopted, but for the cost. This subject is, however, still under consideration by Captain Abney.

January 19, 1893.

The LORD KELVIN, D.C.L., LL.D., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The Bakerian Lecture was delivered as follows:—

Bakerian Lecture.—"The Rate of Explosion in Gases." By Harold B. Dixon, M.A., F.R.S., Professor of Chemistry in the Owens College, Manchester. Received July 8, 1892.

(Abstract.)

1. Berthelot's measurements of the rates of explosion of a number of gaseous mixtures have been confirmed. The rate of the explosion wave for each mixture is constant. It is independent of the diameter of the tube above a certain limit.

2. The rate is not absolutely independent of the initial temperature and pressure of the gases. With rise of temperature the rate falls; with rise of pressure the rate increases; but above a certain *crucial pressure* variations in pressure appear to have no effect.

3. In the explosion of carbonic oxide and oxygen in a long tube,

the presence of steam has a marked effect on the rate. From measurements of the rate of explosion with different quantities of steam, the conclusion is drawn that at the high temperature of the explosion wave, as well as in ordinary combustion, the oxidation of the carbonic oxide is effected by the interaction of the steam.

4. Inert gases are found to retard the explosion wave according to their volume and density. Within wide limits an excess of one of the combustible gases has the same retarding effect as an inert gas (of the same volume and density) which can take no part in the reaction.

5. Measurements of the rate of explosion can be employed for determining the course of some chemical changes.

In the explosion of a volatile carbon compound with oxygen, the gaseous carbon appears to burn first to carbonic oxide, and afterwards, if oxygen is present in excess, the carbonic oxide first formed burns to carbonic acid.

6. The theory proposed by Berthelot—that in the explosion wave the flame travels at the mean velocity of the products of combustion—although in agreement with the rates observed in a certain number of cases, does not account for the velocities found in other gaseous mixtures.

7. It seems probable that in the explosion wave—

(1) The gases are heated at *constant volume*, and not at *constant pressure;*

(2) Each layer of gas is raised in temperature *before* being burnt;

(3) The wave is propagated not only by the movements of the burnt molecules, but also by those of the heated but yet unburnt molecules;

(4) When the permanent volume of the gases is changed in the chemical reaction, an alteration of temperature is thereby caused which affects the velocity of the wave.

8. In a gas, of the mean density and temperature calculated on these assumptions, a sound wave would travel at a velocity which nearly agrees with the observed rate of explosion in those cases where the products of combustion are perfect gases.

9. With mixtures in which steam is formed, the rate of explosion falls below the calculated rate of the sound wave. But when such mixtures are largely diluted with an inert gas, the calculated and found velocities coincide. It seems reasonable to suppose that at the higher temperatures the lowering of the rate of explosion is brought about by the dissociation of the steam, or by an increase in its specific heat, or by both these causes.

10. The propagation of the explosion wave in gases must be ac-

companied by a very high pressure lasting for a very short time. The experiments of MM. Mallard and Le Chatelier, as well as the author's, show the presence of these fugitive pressures. It is possible that data for calculating the pressures produced may be derived from a knowledge of the densities of the unburnt gases and of their rates of explosion.

Presents, January 19, 1893.

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Bronze medal commemorative of John E. Gray, F.R.S., and Mrs. Gray (busts conjoined). Mr. W. T. Thiselton Dyer, F.R.S.

January 26, 1893.

Sir JOHN EVANS, K.C.B., D.C.L., LL.D., Vice-President and Treasurer, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "On the Physiology of the Embryonic Heart (Preliminary Communication)." By J. W. PICKERING, B.Sc., Assistant Demonstrator in Biology at St. Bartholomew's Medical School. Communicated by Professor HALLIBURTON, F.R.S. Received November 25, 1892.

(From the Physiological Laboratory of King's College, London.)

The object of the following experiments has been to study the effect of varying conditions on the heart previous to the development of a nervous mechanism, and thus to throw some light on the discussion as to the relative importance of the two factors in the heart's action, viz., the contractile tissue and the nervous elements. The heart I have used is that of the chick* at a period of incubation of seventy-two hours at a temperature of 38° C. In some cases the embryos have been a few hours older or younger. The embryo is not removed from the egg, but a window is cut 3 cm. square through the shell and shell membrane, exposing the albumen and blastoderm, which remain undisturbed; the egg and embryo is fixed in a small chamber surrounded on five sides by a water-jacket. The uppermost side is covered with glass, while the air of the chamber is kept moist by the evaporation of water from a small bowl placed inside it. The temperature of this chamber can be kept constant or varied at pleasure. My experiments have fallen under three main heads : 1. The results of varying the temperature. 2. The introduction of drugs. 3. Electrical stimulation. In my full paper the results will be shown in tables giving the number of heart beats per minute, the peculiarities in the beat, when such exist, being duly noted. At present, however, I am only prepared to give an abstract of the results obtained, in so far as temperature and drugs are concerned. The electrical experiments are not yet completed.

* Observations are being carried on upon the mammalian embryo *in situ*.

1. *Temperature.*

Each embryo has an individual rhythm of its own, which, if the conditions are constant, remains unaltered, but different embryos, even of the same age, may have different rhythms, so that it is necessary to determine for each embryo its normal rhythm before variations can be studied. An embryo's heart, aged seventy-two hours, at a temperature of 31° C., was beating with a regular rhythm of 84 per minute. The temperature of the air of the chamber was rapidly raised to 42° C., when the rhythm rose to 91 per minute. A further rise to 50° C. increased the rhythm to 128, it still remaining regular. The temperature was then rapidly lowered to 26° C., when the rhythm fell to 114 per minute. A further fall to 16° C. reduced the rhythm to 34 per minute. The temperature was then raised to 46° C., when the rhythm rose to 117° per minute. On again letting the temperature fall to 25° C., the rhythm fell to 36° per minute.

The above experiment, taken as an instance from several, shows that, other factors being constant, the rhythm of the embryonic heart varies directly with the temperature of the surrounding medium.

Extremes of temperature stop the heart; thus exposure to a temperature of 10° C. causes the beats to become weaker and slower, and finally to stop in diastole. If the air of the incubator be raised above 50° C., the beats become so rapid as to be uncountable. They are feeble, and the heart is pale, due to the passage of less blood through it than in the normal state. Violent systolic spasms alternate with periods of quiescence. It stops in an expanded condition when the surrounding temperature is about 55° C. Lowering the temperature restores the beating, but the heart is enfeebled. If the temperature is raised much above this limit the heart is killed. Mechanical stimulation of the heart in standstill, due to either extreme of temperature, if applied at the ventricular end, gives rise to one or more waves of contraction, commencing from the auricular end, and showing the direct conduction through the fibres of the heart. The heart will respond to auricular stimulation when irresponsive to ventricular stimulation. Small variations of temperature, such as one or two degrees, occurring over a long period of time, as in an hour, do not affect the rhythm.

2. *The Introduction of Drugs.*

The drugs employed were applied directly to the heart substance at the temperature of the embryo, and dissolved in normal saline (0·65 per cent. sodium chloride) solution.

a. *Caffeine.*—An embryo, aged sixty-eight hours, at 33° C. had a rhythm of 88 per minute. To its heart 0·00015 gram* of caffeine was

* All weights of drugs used are expressed in grams.

administered, and in two minutes the rhythm rose to 100 per minute, and remained constant for two and a half minutes, when it fell to 96 per minute. A second dose of 0·00015 gram raised the rate to 102 per minute. The beats were also of greater force, since more blood was seen passing through the heart. A dose of 0·0025 gram was fatal. When given to an embryo, aged seventy-five hours, at 37° C., beating with a rhythm of 116 per minute, it reduced the rhythm, after one minute's action, to 100 per minute. The beats, however, remained very strong. After one minute forty-five seconds' action the heart stopped in strong systole, but started again and gave a few powerful beats. After the drug had acted nine minutes thirty seconds the heart stopped permanently in powerful contraction. Caffeine, therefore, acts directly on the cells of the embryonic heart.

b. *Strychnine* was given to a seventy hours' embryo in a dose of 0·000017 gram, and depressed the rhythm of the heart from 112 per minute to 52 per minute. There was no spasm. In an eighty hour embryo, at 39° C., a dose of 0·00002 gram temporarily increased the rhythm, both in force and number of beats; then the systole rapidly became weakened and the rhythm irregular. A further dose of 0·00002 gram still more rapidly reduced both force and frequency of beating, till death in diastole occurred.

c. *Morphine acetate*, if given in doses of 0·0001 gram, is a powerful depressant. With a dose of 0·0002 gram, after one minute's action on an eighty-five hours' embryo at 40° C., irregularities and slowing were obtained; after two minutes' action the beating stopped, but went on again, the waves of contraction sometimes passing from ventricle to auricle, and at others in the normal direction. Periods of rest alternated with violent bouts of rapid beating.

d. *Veratrine.*—Doses of 0·0001 gram increase the number of beats per minute. Larger doses may cause, temporarily, an increase of rhythm, but soon depress the heart by greatly lengthening the systole, which becomes very weak while the diastole is complete. The heart stops in an expanded condition. The heart of a seventy-two hours' embryo that had stopped in diastole, after a dose of 0·0005 gram, was restored by the application of 0·01 gram of potassium chloride almost to its normal rhythm. This agrees with Ringer's observation on the frog's heart.

e. *Potassium chloride*, when applied in a dose of 0·005 gram to an embryo aged seventy-two hours, reduced the normal rhythm of 76 per minute to 60 per minute. A further dose of 0·01 gram reduced the rhythm to 64 per minute. After the administration of a total amount of 0·07 gram of the substance, the heart stopped in diastole.

f. *Nicotine*, in very minute doses, stimulated the embryonic heart; $\frac{1}{4}$ c.c. of a solution containing $\frac{1}{2}$ c.c. of nicotine to 100 c.c. of normal

saline was a stimulant; with $\frac{1}{2}$ c.c. the frequency and force of the heart diminished, systole becoming almost absent, while the heart was finally paralysed in diastole. The addition of 0·03 gram of potassium chloride restored the heart to almost its normal rhythm, the beats at the same time becoming strong, both as regards systole and diastole. A further dose of nicotine depressed the heart, and again brought it into diastolic stoppage, the systoles having become weaker and weaker. There was no spasm.

g. *Atropine.*—Doses of 0·001 gram had, in a sixty hours' embryo, a slightly depressant effect, and even after 0·006 gram had been administered, the rhythm of the heart had only fallen from 96 to 72 per minute. In a seventy-two hours' embryo, with a heart beating at 116 per minute, 0·012 gram, after three minutes' action, had depressed the rhythm to 80 per minute, while even after the administration of 0·275 gram the rhythm was strongly maintained at 64 per minute.

h. *Muscarine Nitrate.*—To the heart of a seventy-two hours' embryo at 35° C., which was beating with a rhythm of 90 per minute, 3 drops of half saturated solution of muscarine nitrate were applied; the rhythm remained constant for two minutes, after which period 2 more drops were added, and the rhythm kept constant at 94 per minute during the next three minutes, after which period 4 more drops were added, and the ensuing rhythm was 93 per minute; 2 drops of saturated solution were then added, which was so concentrated as to stain the embryo brown. During the following five minutes the rhythm was constant at 84 per minute, each beat remaining normal in direction and force. Two more drops of saturated solution caused slight irregularities, but the rhythm during the next seven minutes averaged 72 beats per minute. Finally 2 more drops of saturated solution were added, and during the following seven minutes the heart's rhythm was 75 per minute. The whole experiment lasted thirty minutes, and 10 drops of half saturated *plus* 9 drops of saturated solution of muscarine nitrate were administered. A control experiment with the hearts of two frogs showed that the muscarine used stopped their beats, which were typically restored by atropine. In a similar experiment, witnessed by Professor Halliburton, with both embryonic and frogs' hearts, the rhythm of the former was maintained at 136 per minute, while the latter was stopped and subsequently restored by atropine. Identical results were obtained with a ninety-six hours' embryo. In an embryo aged seventy hours at a temperature of 30° C., which is subnormal in the chick, a rhythm of 92 beats was obtained after the application of 1 c.c. of half saturated solution for the following nine minutes, after which 1 c.c. of saturated solution was applied. This was fatal to the heart, almost instantly coagulating the tissues. There were no typical phenomena

of muscarine poisoning, and the application of atropine failed to restore the rhythm. Probably any strongly alkaloidal body in such a concentrated solution would produce a similar effect.

i. *Schmiedeberg's Digitalin.*—An embryo aged seventy-two hours at 30° C. had a heart rhythm of 132 per minute. To it 1 c.c. of normal saline containing 0·000022 gram of digitalin was applied. During the next eleven minutes the rhythm remained constant, after which time 1 c.c. containing 0·00005 gram was added, which produced no change in the rhythm; then 0·0001 gram was put in, and after one minute's action the frequency of the rhythm had fallen to 92 per minute, but both the systole and diastole were strong. The rhythm after six minutes' action rose to 104 per minute. After this another 0·0001 gram was added, and the rhythm fell after two minutes' action to 50 per minute. The systole was typically perfect, but the diastole was incomplete. The whole heart after two minutes' more action of the drug became very pale and in a state of tonic contraction with very feeble fluttering diastoles, which faded away, leaving the heart stopped in a contracted condition.

j. *Strophanthin* (of Merck's manufacture).—A seventy-two hours' embryo at a temperature of 32° C. had a heart rhythm of 132 per minute. A dose of 0·00006 gram did not alter the rhythm. A second dose of the same amount after twenty minutes' action reduced the rhythm to 54 per minute; both systole and diastole were regular and complete. Five minutes after this the diastole became irregular, and the systole was more marked than in the normal condition. After another minute had elapsed the ventricle passed into a state of tonic contraction, with a few feeble beats, in which the diastole was very weak. The auricles had a marked diastole and a weak systole, and were engorged with blood. During the next five minutes the auricle had a rhythm averaging 24 beats per minute, while the ventricle remained in tonic contraction. Finally, forty-one minutes after the administration of the dose the auricle stopped in diastole, the ventricle remaining in tonic contraction. The auricles responded by 10 beats to a mechanical stimulus; the beats did not extend to the ventricle. Six minutes after this the auricle responded to mechanical stimuli, the wave of contraction passing either from the ventricular end to the auricle or *vice versâ*, according to which end of the auricle the stimulus was applied.

In larger doses of 0·0002 gram the rhythm in a seventy hour embryo at 33° C. was depressed from 120 to 102 per minute, the systole becoming very strong and the diastole imperfect. After four minutes' action the rhythm returned to the normal both in frequency and force. To the same embryo 0·00025 gram was then added, when after one minute's action the auricle dilated, giving small twitch-like contractions, while the ventricle passed into tonic contraction. The

auricle remained for six minutes feebly responsive to mechanical stimuli.

k. *Nitrite of Amyl.*—A ninety-six hours' embryo kept at 35° C. was subjected to the influence of the vapour of 5 minims of nitrite of amyl. After one minute's action the rhythm rose from 96 to 124, and after another minute fell to 112. After another minute it had fallen to 104, and six minutes afterwards was at the normal. In a seventy-two hour embryo at a temperature of 47° the rhythm was 124 per minute. A dose of 1 c.c. of solution of amyl nitrite dissolved in olive oil (strength being 1·5 c.c. of the drug to 10 c.c. of olive oil) was given, and the frequency of the rhythm fell in one minute to 112, but the beats were strong. Six minutes afterwards another c.c. of the solution was introduced, and the rhythm fell to 104, but was strong. Three minutes later another c.c. was put in, and the rhythm rose to 112, but was very weak and irregular, and finally before death the rhythm was reversed.

Concluding Remarks.

The observations here recorded show that the embryonic heart when kept under favourable conditions reacts in a very delicate manner to all those classes of stimuli which influence the adult heart. The experiments on temperature show that its variations act directly on the cardiac muscle, and thus confirm the opinion of Newell Martin* and others who have arrived at the same conclusion from experiments on the adult heart.

The action of caffeine, morphine acetate, potassium chloride, veratrine, nicotine, digitalin, strophanthin, and amyl nitrite is direct on the contractile tissue of the embryonic heart. This greatly favours the view that they act direct on the adult cardiac muscle. It will be noted that many of the actions here described on the embryonic heart are almost identical to those observed by others on the adult heart. Notoriously so is the antagonism between veratrine and potassium chloride, where my observations are identical with those of Ringer† on the frog's heart. A similar antagonism exists between nicotine and potassium chloride. The remarkable correspondence of my results with strophanthin on the embryonic heart with those of Professor Fraser‡ on the frog's heart greatly supports the view of that observer as to the direct action of strophanthin on cardiac muscle without the intervention of any nervous mechanism, and, further, the absence of diastolic stoppages in my experiments also supports Fraser's view that that condition in the frog's heart is due to the

* Newell Martin, 'Phil. Trans.,' 1883, p. 663.

† Ringer, 'Practitioner,' vol. 30 (1883), p. 17.

‡ Fraser, 'Edinburgh Roy. Soc. Trans.,' vol. 36 (1890–91), Part II, p. 388, *et seq.*

action of small doses of strophanthin on the cardiac nervous mechanism of that animal.

The lengthening out of the systole in veratrine poisoning corresponds to the same well-known lengthening of the systole in the frog's heart under veratrine. The reversing of rhythm observed in morphine poisoning is similar to that mentioned by Ludwig* as occurring in the mammalian ventricle when under the influence of opium, for then the auricular beats follow instead of precede the ventricular beats, the rhythm being reversed. The same occurs in amyl nitrite poisoning.

Krukenberg† has stated that neither atropine nor muscarine affects the heart of Ascidians.

My observations on the action of atropine and muscarine, which have been made on a large number of embryos, show that in the absence of a nervous mechanism they do not influence the heart. This will probably modify the current views on the action of these drugs, and my results show that the method I have adopted is a valuable one for differentiating the functions of cardiac muscle from those of the nerves which supply it.

II. "Further Researches in Connexion with the Metallurgy of Bismuth." By Edward Matthey, F.S.A., F.C.S., Assoc. Roy. Sch. Mines. Communicated by Sir G. G. Stokes, Bart., F.R.S. Received November 21, 1892.

In 1886–87 and in 1890 I submitted papers to the Royal Society bringing under notice facts which had come to my knowledge whilst engaged upon the practical extraction of this beautiful metal from its ores, and in its separation from impurities which are always associated with it when in a crude or unrefined state.

IV. *Bismuth, its Separation from Arsenic.*

In a paper dated February 10, 1887,‡ allusion is made to the fact that arsenic is often one of these impurities, and at the same time a method is given by which the separation of this metal from bismuth was then successfully effected.

The process adopted when that paper was read, and for a considerable period subsequently, when working upon bismuth containing arsenic, consisted in removing the arsenic by fusing the arsenical

* Ludwig, 'Lehrbuch der Physiol. des Menschen,' Bd. 2 (1861), p. 38.

† Krukenberg, quoted in Brunton's 'Text-Book of Pharmacology,' &c. (3rd edition, p. 114).

‡ 'Proc. Roy. Soc.,' vol. 49.

bismuth in contact with metallic iron at a dull-red heat and under flux. A compound of iron and arsenic was thus formed and could be removed as a scum; the disadvantages of this process being, 1st, loss of bismuth by volatilisation, and, 2ndly, much loss of time in the manipulation of any large quantity to be treated.

Having occasion, a few months ago, to melt together a large quantity of arsenical bismuth, some 700 or 800 kilos., that is, more than three quarters of a ton, in order to obtain a homogeneous alloy upon which to work subsequently by the process above alluded to, it became evident that when the temperature was raised above the melting point of bismuth, the surface of the metal being exposed to the atmosphere, arsenical fumes appeared, and that these increased as the temperature of the metal became more elevated, the result being that the arsenic came off in dense white fumes (As_2O_3).

The observation of this fact led to further experiments, and it was found that if the surface of the bath of fused arsenical bismuth was freely exposed to the air at a temperature rather higher than its own melting point, and if the molten metal was constantly stirred, it was possible to eliminate the whole of the arsenic alloyed with the bismuth by this simple process of fusion with stirring.

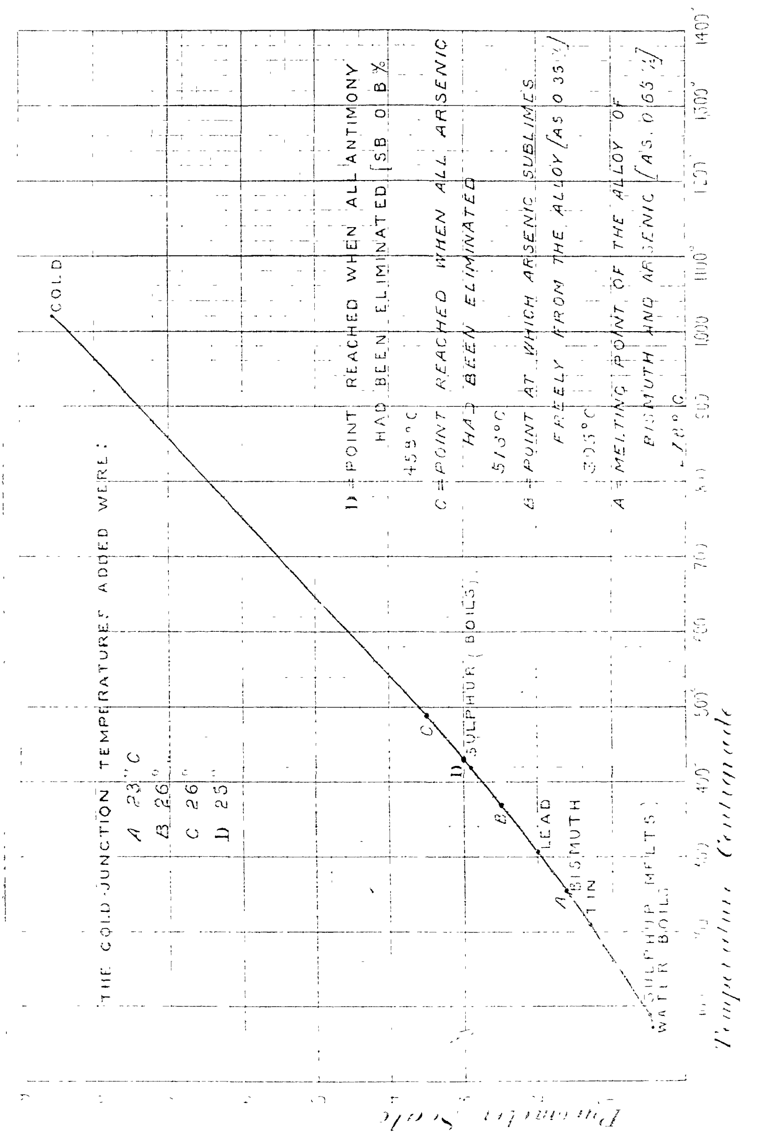
As it is a matter of considerable interest for metallurgists to know, not only that this elimination does take place, but also at what temperature it occurs, a series of experiments have been conducted with a view of determining this accurately.

The work of Roberts-Austen has shown that a thermo-junction is practically the only form of pyrometer that can be used for delicate thermal investigations of this kind, but the question arose which particular thermo-junction should be adopted. Was it well to use the platinum-iridium one as advocated by Barus, or the platinum-rhodium one suggested by H. Le Châtelier? My previous work on the alloys of platinum and rhodium, lately published in the 'Philosophical Transactions,' settled the question in favour of the rhodium-platinum thermo-junction, for I was satisfied that the alloy of platinum with 10 per cent. of rhodium is as homogeneous as any known alloy could well be, and is therefore admirably adapted for use as a thermo-junction, pure platinum being the opposing metal.

The diagram shows the results obtained by calibrating the thermo-junction by the aid of certain known melting points. The temperature at which arsenic is eliminated is also shown on the diagram.

Analysis proved that the alloy operated upon in these experiments contained 0·65 per cent. of metallic arsenic.

From the diagram it will be seen that the melting point of this alloy of bismuth is 278° C. By raising the temperature of the alloy to 395° C. the arsenic freely sublimates from the bismuth alloy, and at a temperature of 513° C. the whole of the arsenic is eliminated.



A point of much interest in relation to molecular physics became evident in the course of the investigation. Arsenic, as is well known, volatilises at the comparatively low temperature of 180° C., without passing through the molten state. Arsenic is not, however,

given off freely from the arsenical bismuth until a temperature of 395° is reached. So that the temperature of disassociation of this alloy, containing 0·65 per cent. of arsenic, is 114° C. higher than the melting point of the mass. It was interesting to determine at what temperature the arsenic would be evolved if the alloy were heated *in vacuo.*

A portion of the bismuth alloy containing 0·65 per cent. of arsenic was introduced into a hard glass tube, slightly depressed to its centre, and connected at one end with a Sprengel pump, by which it could be rendered vacuous.

The pyrometric wires were in contact with the metallic alloy and passed to the galvanometer through the opposite sealed end of the glass tube.

Heat being applied, the first indication of the volatilisation of the arsenic, shown by the condensation of a film on the cool part of the tube, occurred at 275° C. When the alloy was quite melted the temperature indicated was 316° C.; arsenic came off freely when the temperature rose to 569° C., condensing in a black mirror.

The metal was then allowed to cool, and its setting point was found to be 268° C., which corresponds with that of the melting point of bismuth.

The bulk of the arsenic does not appear to be evolved *in vacuo* at a lower temperature than in air.

As regards the industrial application of the process, some ten to twelve tons of arsenical bismuth have already been treated in this very simple way, and it has been satisfactorily ascertained that there is no loss of bismuth by volatilisation with the arsenic.

Part V.—*The Separation of Bismuth from Antimony.*

The process hitherto adopted in practice for the separation of antimony from bismuth has usually consisted in a simple fusion at a dull-red heat with bismuth oxide or bismuth "litharge"—an operation successful enough as to its results, but one requiring no small amount of skill in manipulation; it is also one by which only small quantities can be treated readily at one time—and moreover, the temperature which is necessary to effect the separation of the antimony involves appreciable loss on account of the volatilisation of the bismuth at a red heat, notwithstanding many tens of tons of bismuth have, however, been treated under my direction by this process.

In an operation lately conducted, involving the melting of a quantity of bismuth containing about one per cent. of antimony, it was found that a peculiar oily film was noticeable rising to the surface of the melted alloy; this film did not form all over the surface of the

metal, but appeared to rise as from a boiling centre, and this although the metal was at a temperature very little above its melting point. A portion of the film or layer was removed and tested in order to ascertain its nature, and it was found to contain a very appreciable proportion of *antimony*. I, therefore, caused the operation to be continued, stirring the metal from time to time with a dried wood stirrer. In the course of three or four hours, removing the film from time to time, the surface of the melted metal assumed a much brighter appearance, and on carefully testing it at this point, the metal was found to be *absolutely free from antimony*. To confirm this and to ascertain more exactly the conditions under which this separation takes place, a further quantity of impure bismuth was operated upon in a similar manner. This second quantity contained other impurities besides antimony, its analysis being as follows:—

Bismuth, by difference	96·20
Antimony	0·80
Tellurium	0·40
Lead	2·10
Copper	0·50
Arsenic	traces
	100·00

The same simple process of fusion and stirring was again adopted —the quantity being about 350 kilogrammes—and when the same oily film commenced to rise to the surface the temperature of the molten mass of the alloy was taken by means of the Le Chatelier pyrometer. A portion of the film removed showed, on being tested, a percentage of over 30 per cent. of antimony. A slightly perceptible fume of arsenic was apparent as volatilising, so proving what I found to be the case in the separation of arsenic by simple fusion. (See *ante*.)

The point at which this separation of antimony occurs was found to be 350° C., and at this temperature the metal was maintained for about five hours.

The evidence of an oxidising action became now much less, and, although a very small amount of antimony was present, there was still a little remaining in the alloy; the temperature, therefore, was slightly raised and maintained at 458° C., as shown by the pyrometer, for about four hours, at the end of which time the bismuth became absolutely free from antimony.

The form in which the antimony separated was peculiar—a transparent glass, consisting of antimony oxide—containing about 10 per cent. of bismuth, but of course in the removal of the antimony oxide a small proportion of the bismuth was mechanically carried with it,

resulting in the production of several very interesting and very beautiful metallurgical specimens.

The great advantage of this process is, like that of the foregoing separation from arsenic, its extreme simplicity, the low temperature which renders it possible to work upon very large quantities at one time, and the very small amount of time necessary for this separation in comparison with the process hitherto adopted, and the absence of loss in the bismuth operated upon by volatilisation. It is obvious that where metals can be so easily treated in large quantities, the labour and skill hitherto necessary is very considerably reduced, and there is the additional advantage that the loss attending large operations is minimised.

In this and in my previous papers upon this beautiful metal bismuth, I have been able to point to simple dry processes for its separation from gold, lead, copper, arsenic, and antimony, and all these processes are available for treating with care large quantities at one time. When it is remembered what is involved in having to dissolve any quantity of bismuth in acids, and its subsequent precipitation from solution, it surely will be admitted that much of the difficulty in purifying crude bismuth has been effectively removed, as the methods given have been found possible in practice, and advantageous.

I have introduced upon the diagram the points at which arsenic is volatilised, and also the point at which antimony separates from bismuth under the conditions described in this paper.

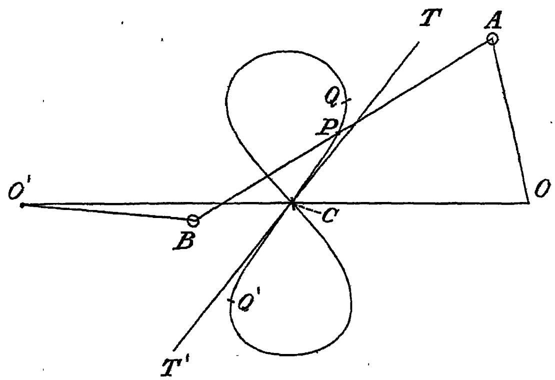
III. "On the Three-Bar Motion of Watt." By WILLIAM BRENNAND. Communicated by C. B. CLARKE, F.R.S. Received January 2, 1893.

(Abstract.)

The figure represents a simple form of "Watt's Parallel Motion." $OA = O'B = r$ are the arms that can turn freely about O,O′, fixed centres, in one plane. The link $AB = 2l$ is pivoted at A and B. As the arms move, P the middle of the link, traces out a portion of the curve, viz., from Q to Q′, backwards and forwards, nearly in a right line.

$OC = O'C = d$.—Of the three parameters d, r, l, any one can be taken as unit (in this paper l is taken 10 units); then d and r are independent parameters. The problem Watt had to solve was to discover numerical values of d and r that should give the tracing point P the smallest deviation from a right line.

Watt gave a series of values for d and r which are employed by engineers, with small thumb-rule ameliorations, to this day. They are



"good" values, *i.e.*, they give the path of P nearly rectilinear; but they are all subject to the relation $d^2-r^2=l^2$. The curve above is one of this class; in these the tangent at the origin lies wholly outside the curve, and has the closest possible contact with it, while at the moment the tracing point P passes through the origin, the arms are at right angles to the link. The question arises, may there not be better values of d and r (not subject to the relation $d^2-r^2=l^2$) which give a more nearly rectilinear motion to P than any of the Watt values?

The equation to the curve traced by P, in common polar coordinates, is (taking CO initial radius vector)—

$$(\rho^2+l^2-r^2-d^2)^2+4d^2(\rho^2-r^2)\sin^2\theta=0 \;\ldots\ldots\; \text{(A)}$$

Willis ('Principles of Mechanism,' p. 401) says that the full equation is so exceedingly involved and complex as to be of no use in obtaining the required practical results. And Willis accordingly follows the preceding writers in "approximate methods."

The present paper takes up the subject at this point, and the general substance of the paper and its results may be stated under three heads, viz.:—

1. The nature and properties of the curve (A) are worked out so that a complete idea of it for all values of d and r is obtained.

2. Hence are derived numerous values for d and r which give good results; the deviation in these from the right line is calculated, and in some of them shown to be less than in any of the arrangements given by Watt.

3. The more complex arrangements, where the radii are not equal or where the tracing point divides the rod unequally, are also dealt with.

In the first head, large use has been made of plotting the para-

meters d and r as if they were x and y coordinates, and supposing at each point of the "chart" thus resulting that the curve (A) is existent at that point.

This method is capable of great extension; it is used in the present paper in the heading (3), and has been found of great service in dealing with other complex curves, as when the tracing point P is not in the straight line AB.

Watt gave a rule for "fixing" in the particular set of cases where $d^2-r^2=l^2$. A very simple practical rule for fixing has been found for all values of d and r.

The 'English Mechanic' of December 29, 1882, published my first attempts on this curve; and the chart is there employed.

The numerous writings of late years on Three-Bar Motion, by Mr. Roberts, Professor Cayley, and others, do not appear to invade the narrow area of the present paper, which especially aims at obtaining numbers for d and r of practical value.

Presents, January 26, 1893.

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Zigno (Achille de) Cenni Biografici estratti dal Discorso d'Apertura della Riunione della Società Geologica Italiana. Letto dal Prof. G. Omboni. 8vo. *Padova* 1892. Prof. Omboni.

Annotated author's copy of Sir Charles Bell's "Idea of a New Anatomy of the Brain." 8vo. [*London* 1811.] Privately printed. Prof. F. Jeffrey Bell, M.A.

Bronze "Comet Medal," in the gift of the Astronomical Society of the Pacific. Lick Observatory, California.

February 2, 1893.

Sir JOHN EVANS, K.C.B., D.C.L., LL.D., Treasurer, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "A New Portable Miner's Safety-lamp, with Hydrogen Attachment for Delicate Gas-testing; with Exact Measurements of Flame-cap Indications furnished by this and other Testing Lamps." By FRANK CLOWES, D.Sc. Lond., Professor of Chemistry, University College, Nottingham. Communicated by Professor ARMSTRONG, F.R.S. Received January 10, 1893.

Introductory.

The presence of firedamp is still almost invariably detected, and its amount is more or less accurately measured, by the observation of the pale blue "cap" produced by the "gas" over the flame of the safety-lamp. By suitably reducing the flame of an ordinary oil safety-lamp by drawing down the wick, it is generally considered that percentages of gas in the air varying from about 2·5 to 6 can be detected and estimated. The upper limit is that at which an inflammable mixture is approached. The lower limit reached by this method of testing is, however, not considered sufficiently low, and for two reasons. W. Galloway has shown ('Roy. Soc. Proc.,' vol. 24, p. 361) that air containing less than 1 per cent. of gas becomes explosive when it is mingled with fine coal-dust; hence the ordinary safety-lamp test for gas is incapable of indicating a dangerous state of the atmosphere in dusty mines. And, further, even in the absence of coal-dust in the air of the mine, it is necessary to test the "returns," or the ventilation currents as they leave the working "districts" and the mine, in order to ascertain whether the general ventilation of the mine is well distributed and sufficient. It is stated by high authorities that the "main return" air should not contain more than 0·5 per cent. of "gas," and that 1 per cent. should never be reached. An ordinary oil safety-lamp gives no indications with 1 per cent. or less of gas.

During the last twelve years several forms of gas-testing apparatus

have been introduced with the object of enabling percentages of gas as low as 0·5, and even as 0·25, to be determined. The lower proportion, 0·25 per cent., is considered to be low enough for all practical purposes.

Of these apparatus, the following are considered to effect their purpose satisfactorily: E. H. Liveing's electrical indicator ('Physical Soc. Proc.,' June, 1880): Fr. Pieler's alcohol-lamp, described in a pamphlet ('Ueber einfachen Methoden zur Untersuchung der Grubenwetter,' Aachen, 1883): and an apparatus brought forward by Coquillon and by others, which depends upon measuring the reduction of pressure produced in a confined volume of the mine-air when the firedamp is burnt out of it, this being effected by maintaining the air in contact with a metallic wire rendered incandescent by an electric current.

Apart from general considerations of convenience and of safety when these apparatus are in use in the mine, a serious objection to each of them is that it is by no means small or light, and that it must be carried together with an ordinary safety-lamp, since it does not itself serve for illuminating the darkness of the mine.

The Liveing apparatus has recently been proved by James Grundy, working with my test-chamber, to give very accurate readings of "gas" varying in percentage from 0·11 to 2·2; but he found the platinum wire exposed to the gas to be subject to changes when in use, which make the apparatus difficult to maintain in working order and which sometimes render it useless.

There seems to be a general disposition amongst all classes interested in mining to improve, if possible, the flame-cap test, rather than to resort to other methods for securing accuracy and delicacy in gas-testing. This object was in some measure attained by MM. Mallard and Le Chatelier in 1881 ('Annales des Mines,' 7th Ser., vol. 19, p. 186), by suitably screening the reduced oil flame of the safety-lamp, and then viewing its tip against a black background of cloth or of blackened metal sheet. It is stated that a slight indication was obtained by this means when the percentage of gas present was as low as 0·5. The lamp, however, suffered a loss of illuminating power when its flame was turned up, owing to the obstruction of the screens. The indications of the lower percentages were also confessedly extremely slight and feeble; they are further rendered uncertain by the fact that an oil-flame itself gives a feeble cap-like mantle. Still the improved lamp has the advantage of being at once an illuminator and also a gas-tester of greater delicacy than the lamps in ordinary use.

The inventors of this improved oil-lamp state that they consider the hydrogen flame to be superior to any other for gas-testing. They ascertained that this flame would detect 0·25 per cent. of gas; but

they do not publish any further account of its indications, apparently because their attempt to introduce the hydrogen flame into a safety-lamp was altogether unsuccessful.

Pieler was also so much impressed with the advantages obtained by the use of a hydrogen flame for gas-testing, that, failing to apply this flame to a portable lamp, he still recommended (*loc. cit.*) the application of the hydrogen flame, fed by a chemical generator, in a stationary apparatus above ground, to testing samples of mine-air which were conveyed to it. He confesses that his alcohol-lamp is only a substitute for the impossible hydrogen-lamp, which he would evidently recommend if it were available. The alterations recently made by M. Chesneau ('Annales des Mines,' August, 1892, p. 203) in the Pieler lamp would therefore probably not have been recommended by the original inventor of the alcohol lamp, if a portable hydrogen lamp had been in existence; and although they add to the safety of the lamp, they do so by rendering the lamp self-extinguishing in moderately high percentages of gas. The extinction of the testing-lamp is a serious inconvenience to the gas-tester, and will not prove to be a recommendation to him.

The Pieler lamp further labours under the serious disadvantage that the pale caps have to be observed through wire gauze, which obstructs much of their light. The caps become actually invisible if the gauze around the lamp is bright and reflective, as it frequently is in a new lamp.

When samples of air can be conveniently collected and carried to a testing station, no arrangement will be found more convenient and delicate than a hydrogen flame of standard size, fed by a large cylinder of the compressed gas. The flame-cap is then observed in a dark room against a dead-black ground, and its height is noted. A portable hydrogen safety-flame will, however, be found to be much more convenient, as it can be carried to the spot where the test is to be made. The troublesome collection and transport of bulky air samples are thus avoided.

A Portable Safety-lamp with interchangeable Oil and Hydrogen Flames.

An arrangement for introducing a standard hydrogen flame into any ordinary form of safety-lamp has been already described ('Roy. Soc. Proc.,' vol. 51, p. 90). The requisite supply of hydrogen was contained in a compressed state in a steel cylinder, which was similar to those in common use, but of much smaller dimensions. The cylinder was slung from the shoulder by a strap, and was connected when necessary with the lamp by means of a length of flexible tubing. On slightly opening the valve of the cylinder, the

hydrogen was fed through a copper tube of very fine bore within the lamp to a jet terminating beside and just above the wick of the lamp. This copper tube passed through the oil reservoir of the lamp.

The hydrogen having been kindled by the lamp-flame, the wick was drawn down by the "pricker" until the oil-flame was extinguished. The hydrogen flame was then set to a standard height of 10 mm. by regulating the very tapering cylinder-valve, whilst looking across a vertical 10 mm. wire fixed on the wick-holder in front of the hydrogen flame. The flame-cap was then looked for against a dead-black background, produced by smoking with a taper a vertical strip half an inch wide upon the interior of the back of the lamp-glass. The lamp-glass was specially made of greater length than usual, so as to enable the whole of a flame-cap 60 mm. in height to be seen. The height of any flame-cap thus observed was estimated by comparing it with the standard 10 mm. hydrogen flame, or, if the flame had increased in height, by taking the vertical wire as the fixed 10 mm. standard of measurement.

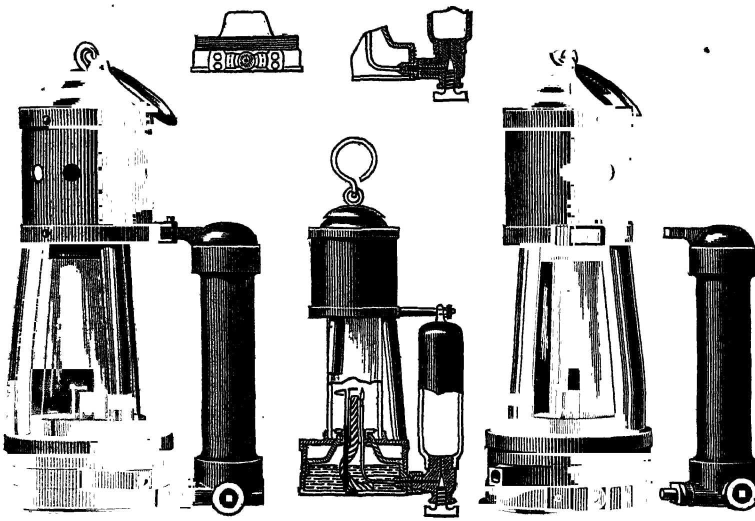
When the observation of the flame-cap had been completed, the lamp-wick was pushed up once more against the hydrogen flame; and as soon as the oil-flame had been kindled, the hydrogen was shut off by the valve of the cylinder, and the flexible tube was disconnected from the lamp.

The whole process of producing the standard hydrogen flame within the ordinary safety-lamp was very simple and was very rapidly effected; and the change from the oil flame to the hydrogen flame, and the converse, presented no difficulty. A little practice enabled the operator to turn on the hydrogen sufficiently slowly, if the valve was properly constructed; and a little care sufficed to prevent the extinction of the hydrogen flame by sudden shocks imparted to the flexible connecting tube.

This form of apparatus appears to be suitable for many purposes. But practical mining men objected to the unnecessarily large weight and dimensions of the hydrogen cylinder, and to the inconvenience of the connecting tube. For making one's way along rough and awkward passages underground something more compact and light was requisite.

After many experimental forms of apparatus had been tried, a little steel cylinder directly and rigidly attachable to the safety-lamp was adopted. It is shown attached to Ashworth's modified lamp in fig. 1. The sectional drawings represent the key of the valve below the cylinder; it is in reality at the side and is detachable; the attachment of the cylinder to the lamp is also not accurately represented in the sections. This cylinder may be adapted to any other form of safety-lamp, but for several reasons Ashworth's lamp has been pre-

FIG. 1.



ferred. The cylinder is made of hydraulic steel tube; it is 5 inches in length by 1 inch in diameter, and barely exceeds 1 lb. in weight. It can be instantaneously attached to the lamp by a quarter turn; a clip arrangement then holds it firmly at both ends; it is as quickly detachable. When the cylinder is not attached, the lamp is an ordinary safety-lamp. The cylinder, when attached, forms a most convenient strong and rigid handle at the side of the lamp, by which the lamp may be supported in the left hand while the hydrogen flame is being adjusted by the right. The whole process of passing from the bright flame to the hydrogen flame and back again to the bright flame, including a hydrogen-flame test, is easily effected in thirty seconds. The process of testing is precisely that described above for the cylinder with flexible lamp connexion.

The little cylinder is charged with hydrogen by connecting it with a larger store cylinder of the gas under a compression which may vary between 120 and 60 atmospheres. When charged at 100 atmospheres pressure, it furnished the standard 10-mm. flame, burning continuously, for about 40 minutes. This would be an ample supply of hydrogen for one inspection of a mine, since the hydrogen flame would only be used occasionally, and then for very short intervals only. If a longer service is required, a cylinder of the same diameter, but of greater length, and of capacity half as great again, may be adapted to the lamp. Or one or more reserve cylinders can be easily carried in the pocket. The small cylinder may be charged sufficiently for use several hundred times from a large cylinder under 120 atmospheres pressure.

When "gas" is being tested for in the mine, the examination would first be made in the usual way by carefully and gradually drawing down the wick, watching continuously meanwhile through the well-cleaned glass, with the dead-black smoked background already described, whether at any stage a cap appears over the flame. If a distinct cap within the limits of the glass should be seen, the percentage of gas will be between 3 and 6; by noting the height of the cap as judged against the standard 10-mm. wire, the percentage of gas can be ascertained with very fair accuracy, as is proved below.

If no satisfactory cap can be seen over the reduced oil flame, and small percentages of gas have to be looked for, the hydrogen cylinder is attached to the lamp, the standard hydrogen flame is produced within the lamp, and a cap is looked for over the flame. This standard flame will readily indicate and measure percentages varying from 0·25 to 3: the caps being easily seen and readily measured by comparing their heights with that of the hydrogen flame or of the standard wire. The cap corresponding to 0·25 of gas is very pale, and is somewhat hazy and ill-defined at its edge; the cap with 0·5 per cent. is somewhat more dense and well-defined; but both these caps are easily seen even by an inexperienced eye.

The dimensions of the caps given by less than 1 per cent. of gas can be much increased by turning up the hydrogen flame to 15 mm. (see H_1 H_1, figs. 5, 6). Similarly, the cap indications of the standard hydrogen flame, which exceed the height of the lamp-glass when more than 3 per cent. of gas is present, can be brought within the visible limits by reducing the flame, while in the presence of the gas, to 5 mm. (see H_2 H_2, figs. 5, 6). These higher percentages of gas may in this way be estimated by means of the hydrogen flame, instead of by the reduced oil flame.

It has been occasionally noticed that, when the hydrogen flame is allowed to burn for a long time within the lamp, the lower part of the lamp-glass becomes cool, owing to the feeble radiative power of the flame; drops of water then condense upon the glass and interfere with the accurate observation of the flame and of the cap. If this should occur, it is only necessary to turn on the oil flame for a short time; and this, by its superior lateral radiation, rapidly dissipates the water.

It will be readily understood that the main advantages resulting from the use of the hydrogen flame are the following:—

1. The flame is non-luminous, whatever its dimensions may be, and therefore does not interfere with the perception of the cap, and does not require to be screened from the eye.
2. The flame can always be adjusted at once to standard height, and

maintained at that height sufficiently long for the completion of the test; whereas other testing flames are constantly varying in dimensions, and most of them cannot be set to standard size at all with any certainty.

Thus a colza-petroleum flame, exposed in air containing a low percentage of gas, when twice adjusted, gave caps of 8 and of 20 mm. The reduced oil-flame also fell so quickly that cap-readings with low percentages of gas could not be taken at all.

3. The caps produced over the hydrogen flame are larger than those produced by any flame of corresponding size.
4. The size of the hydrogen flame can therefore be so far reduced as to enable it to be used in an ordinary safety-lamp.

   The size of the flame may further be suitably varied, so as to increase or decrease the height of the cap, and thus either increase the delicacy of the test or extend its range.
5. The standard hydrogen flame shows no trace of mantle or cap in air free from gas; it shows only a slender thread above its apex. The colza-petroleum and the benzoline flames show pale mantles in gas-free air, which may be easily mistaken for a small percentage of gas.
6. The standard hydrogen flame burns vigorously, and is of fair size, and therefore cannot be extinguished by accident; whereas the reduced flames, ordinarily used in testing, burn feebly and are readily lost.
7. Hydrogen is supplied fairly pure and of practically invariable composition; whereas commercial oil and alcohol are apt to vary much in composition, and therefore to give flames whose indications change with the sample of liquid which is being burnt.

The Precise Measurement of Flame-caps.

The flame-caps in a long series of observations, of which the results are here stated, were observed in the test-chamber already described ('Roy. Soc. Proc.,' vol. 50, p. 122 and vol. 51, p. 87). This apparatus has been made more convenient for use by slinging the water-tray, which effects the closure of the chamber below, from the bottom of the chamber by four rods which furnish a kind of parallel motion. The tray can thus be pushed back and again brought into position, and be maintained throughout in a horizontal position. It has also been found necessary to introduce a small circular trap-door in the floor of the chamber, and just behind the window. When an oil-lamp is placed over this opening, its flame can be readily adjusted by the "pricker" from outside, without removing the lamp from the

chamber. The chamber was found to clear itself of gas in two minutes when both top and bottom apertures were left open. With the bottom aperture only opened, it stood for five minutes without appreciable change occurring in the composition of its internal atmosphere.

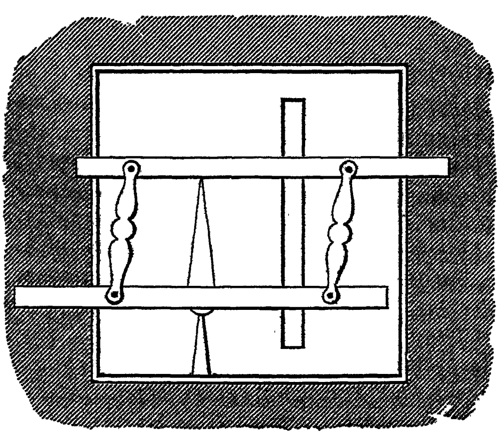
Precautions were always taken, before commencing the measurements of caps, to make the eye as sensitive as possible, more especially when observing the paler caps. This was effected by remaining in a dark room for some time. The interior of the test-chamber and the back of the lamp-glass received a dead-black coat: and advantage was gained by coating in the same way all those brass parts of the lamp which could receive and reflect the light of the flame or cap. Very feeble daylight was found to cause much more serious interference than full gaslight did, with the observation of the cap.

A suggestion made by Professor H. B. Dixon was found of great advantage when examining feeble caps; the eye was directed on the side of the lamp, instead of directly upon the cap; the pale image of the cap was thus formed upon a very sensitive part of the retina, and the cap was much more readily seen than when it was observed directly.

All attempts to measure the heights of flame-caps by means of graduated scales fixed within the lamp ended in failure, since the light emitted by the cap near its summit was far too feeble to illuminate the scale. The method of measurement ultimately adopted, which has served for making many hundred readings, is described below.

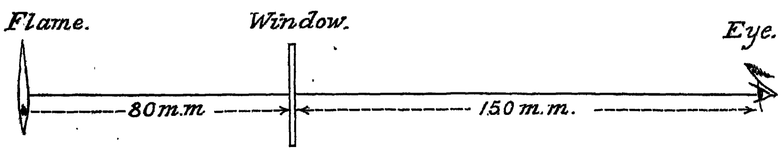
An ordinary flat parallel-ruler was pressed against the window of the chamber in a horizontal position (fig. 2), and supported a vertical

FIG. 2.



strip of paper between it and the glass. The flat front surfaces of the rule were rendered feebly luminous by means of luminous paint, so that their position could be seen in the dark. The rule was then carefully adjusted so as to exactly include the cap between its inner edges: the cap being considered to extend downwards from its apex to the tip of the flame. The position of the inner edges of the rule were then marked upon the paper strip by means of a sharply pointed pencil; and the distance thus marked was read off on a millimetre rule. This height was manifestly less than the height of

FIG. 3.



the cap, and was corrected by multiplying it by the distance between the eye and the cap (230 mm.), and dividing the product by the distance between the eye and the glass (150 mm.).

It may be mentioned that every reading was made in duplicate; most readings were repeated many times, and in many cases were taken by more than one observer. Successive readings of the cap produced by the standard hydrogen flame in the same percentage of gas were frequently identical, and rarely differed by more than 1 mm. In the case of flames which could not be brought with certainty to an invariable standard condition, the readings were naturally less constant, but this arose from no fault in the method of measurement adopted.

The lamps used for testing should be kept clean. Small openings protected by gauze must, if above, be especially kept free from obstructive deposits of soot from the flame; and, if below, must be cleansed from films of oil, which may close the meshes of the gauze entirely.

It is noticed generally, that the caps produced by low percentages of gas are pale and wanting in density and definition. As the percentage of gas increases, the cap gains in size, in density, and in definition, and wraps round the flame to its very base. The testing flame also increases gradually in dimensions as the percentage of gas rises, and in the presence of 3 per cent. of gas and upwards the testing flame becomes more bulky, growing both in height and in girth. It is therefore important to note that the 10-mm. hydrogen-flame was adjusted in gas-free air, because its size did not materially alter until over 3 per cent. of gas was present; the 15-mm. and 5-mm. flames,

however, were adjusted in the presence of the "gas" which was being estimated.

Results of Flame-cap Measurements in Air containing Methane (Firedamp).

The gas used for these experiments was prepared by the usual chemical method, which consists in strongly heating an intimate mixture of dried sodium acetate with caustic soda and slaked lime. This product cannot claim to consist of pure methane, but, judging by the constant results which it yielded in the test-chamber, the gas must at least have been of fairly constant composition.

The lamp-flames used were of the following dimensions, some of which are approximate only. They are drawn to size in fig. 4, the dimensions being shown in millimetres and in inches :—

	Height in mm.	Diameter at broadest part in mm.
Hydrogen flame (round)	10	5
	15	6
	5	4
Pieler flame (round, conical)	30	13
Benzoline flame (round, conical)	3	7
Oil flame (flat, slightly conical)	3	13
	6	13

The 10-mm. hydrogen flame was adjusted to its height in gas-free air; it remained unaltered in height until 3 per cent. of gas was reached, when its height increased by from 2 to 3 mm.

The 15-mm. and 5-mm. hydrogen flames were adjusted to height in the presence of the gas.

The Pieler, the benzoline, and the blue oil-flame were each of them adjusted in gas-free air. The faintly luminous oil-flame was adjusted in the air containing gas, the bright flame being gradually reduced until the cap was seen to be of the largest possible dimensions.

The following are the corrected cap-measurements expressed in millimetres, obtained with the flames of the specified heights; the caps are graphically represented in millimetres and in inches in figs. 5, 6, 7, 8. In these drawings the increase in dimensions of the testing flames, as the percentage of gas rises, is not shown. All the flames, with the exception of the Pieler alcohol flame, were tested in the Ashworth lamp.

Percentage of methane.	Hydrogen flame.			Pieler alcohol flame, 30 mm.	Ashworth's benzoline flame, 3 mm.	Colza-petroleum flat flame.	
	Standard 10 mm.	15 mm. in the gas.	5 mm. in the gas.			Small blue, 3 mm.	Flame partly luminous, 6 mm.
0·25	17	37	—	30 (?)	—	—	—
0·5	18	42	—	55	7 (?)	—	—
1·0	22	60	—	90	10	—	—
2·0	31	enters top of lamp	—	140, reaches top	14	7·5	7·5
3·0	52	—	14·5	—	20	7·5	7·5
4·0		—	22·2	—	25	12·0	24·0
5·0	enters top of lamp	—	35·0	—	30	29·0	41·0
6·0	—	—	60·0	—	35	67·0	enters top

In cases where caps of equal height are given for different percentages of gas, it must be understood that the higher percentage gives a more dense and sharply defined cap than the lower percentage does.

The general conclusions drawn from these measurements, and from experience derived from working with the different lamps, are the following:—

1. The indications of the Pieler lamp begin at the lowest limit of 0·25 per cent., but quickly become too great to be utilised. The thread-like tip extending above the flame for several inches in pure air must not be mistaken for a cap, but it is scarcely distinguishable from the cap given by 0·25 per cent. of gas.

   This lamp suffers under the disadvantage that much of the feeble light of the caps is lost by the obstruction of the gauze: the gauze also frequently presents a bright reflecting surface behind the flame, and this renders the observation of the cap impossible. All the other lamps in use are free from the interference due to the gauze, and if their glasses are blackened, as already described, they become well suited for the observation of caps.

2. The Ashworth benzoline lamp begins its indications doubtfully at 0·5 per cent., the cap thus produced being more distinct, but not greater in height, than the mantle of the flame seen in gas-free air.

   But starting with certainty with an indication of 1 per cent.,

FIG. 4.—Size of Lamp-flames.

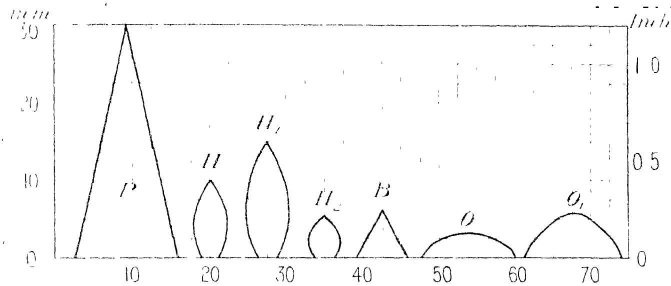
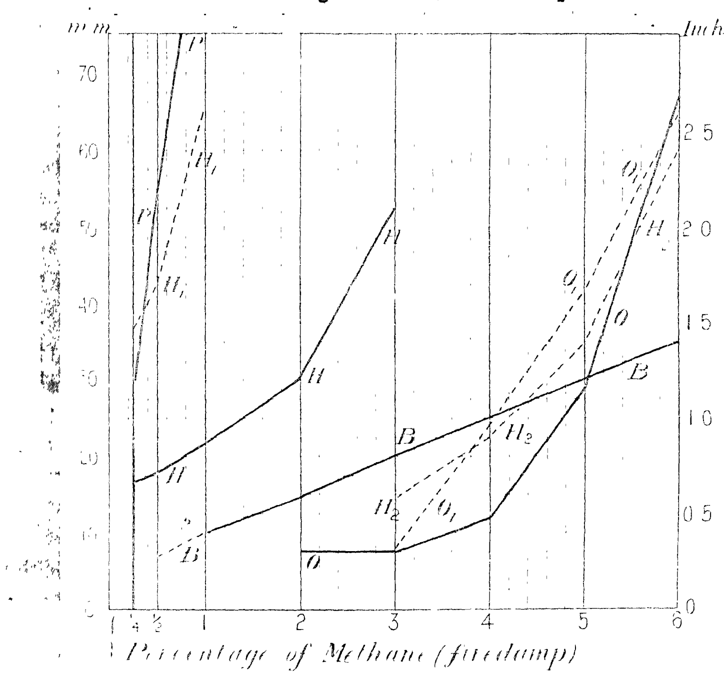


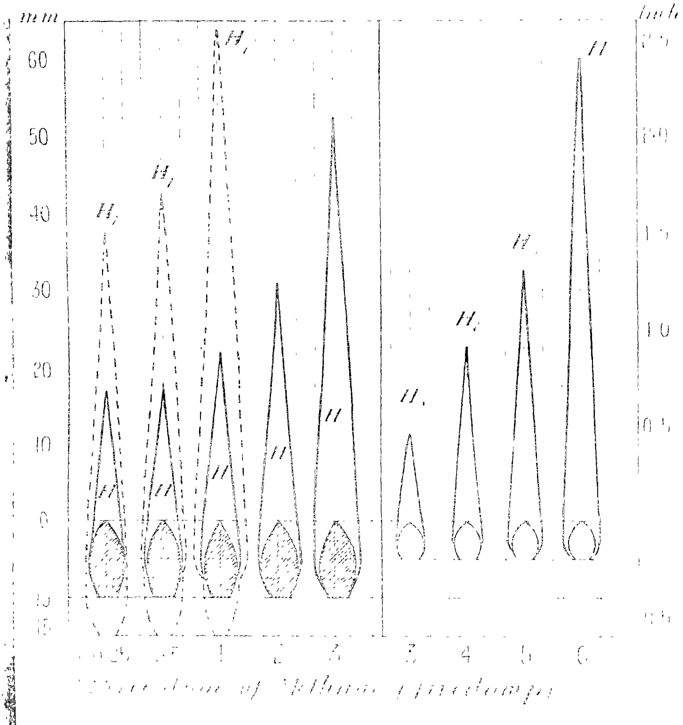
FIG. 5.—Heights of Methane Flame-caps.



O = Colza-petroleum flat flame (blue).
O_1 = " " (luminous tip).
B = Ashworth's benzoline-flame (blue).
H = Hydrogen flame, standard 10 mm.
H_1 = " 15 mm. in the gas.
H_2 = " 5 mm. in the gas.

FIG. 6.

FIG. 6.—Actual Dimensions of Hydrogen Flames and Caps with Methane.



The shaded flames are of the standard 10 mm. size; their caps are shown at (H).
The dotted flames and caps (H_1) correspond to the flame 15 mm. high in the gas.
The three figures to the right (H_2) represent 5 mm. flames and their caps; flames set in the gas.

it gives strikingly regular indications up to 6 per cent., and even higher percentages may be read off in a lamp with a long glass.

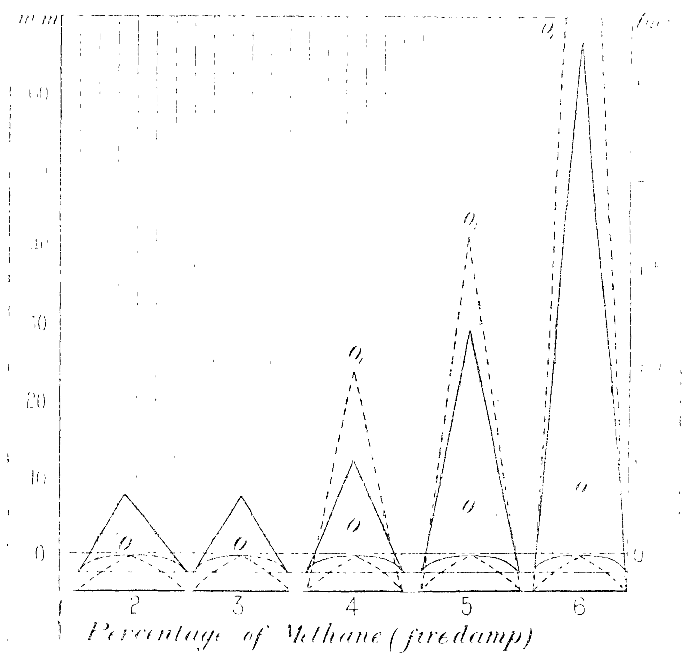
3. The standard 10-mm. hydrogen flame gives distinct indications from 0·25 to 3 per cent.; the cap then becomes too high for measurement in the lamp; but by reducing the flame to 5 mm., cap-readings may be taken up to 6 per cent. of gas.

   The lower indications may similarly be increased by raising the flame to 15 mm.

4. The oil flame, produced by unmixed colza oil, gives no indications with percentages below 2. With 1 per cent. of gas the flame from colza mixed with an equal volume of petroleum

FIG. 7.—Actual Dimensions of Benzoline Flame and Caps.

FIG. 8.—Actual Dimensions of Oil Flames and Caps.



The continuous lines represent small blue flames and their caps.

The dotted lines represent caps produced when the flame was adjusted so as to give maximum caps.

(water-white) produces an apparent cap, which, though somewhat more intense than the natural mantle seen in gas-free air, is only equal to this mantle in dimensions, and might easily be mistaken for it.

The oil flame, when it is reduced until it just loses its luminous tip, however, gives distinct indications from 3 to 6 per cent.

The largest indications are produced by drawing down the flame *in the presence of the gas*, until a cap of maximum size is obtained.

A carefully regulated oil flame may, therefore, conveniently supplement the hydrogen flame for the indication of gas varying from 3 to 6 per cent., and in the new hydrogen lamp this will be found to be a convenient method to adopt.

The use of colza alone in the oil-lamp is very inconvenient for gas-testing: the wick quickly chars and hardens on the top, and the flame cannot then be reduced without danger of extinction; the flame can never be obtained satisfactorily in a non-luminous condition. The admixture with petroleum obviates these difficulties.

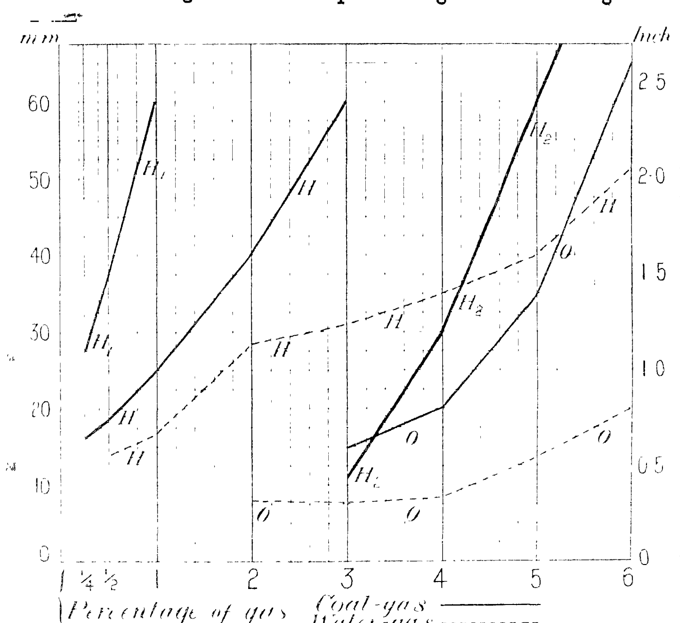
Results of Flame-cap Measurements in Air containing Coal-gas.

The miner's safety-lamp is frequently employed for purposes of illumination in gas-works, and in other places in which the air may be contaminated with coal-gas. It seems, therefore, only reasonable to make the lamp also serve the purpose of detecting the percentage of coal-gas present. The amount of gas thus found indicates whether the air is in a dangerous condition. Careful measurements were accordingly made of the flame-cap heights seen over the hydrogen and oil flames of the safety-lamp, when it was placed in the test-chamber with air containing a known admixture of Nottingham coal-gas.

The following measurements in millimetres were obtained. They are represented graphically in millimetres and in inches in fig. 9, but no attempt is made in these drawings to show the marked increase in dimensions which the test-flame undergoes in the presence of the higher percentages of gas :—

Percentage of of coal-gas.	Cap-height in mm. over hydrogen flame.			Cap-height in mm. over oil flame, reduced until the cap is at maximum.
	Standard 10 mm. flame.	15 mm. flame.	Flame reduced to 5 mm. in the gas.	
0·25	15·7	27·6	—	—
0·5	18·4	37·0	—	—
1·0	25·3	60·4	—	—
2·0	40·6	—	—	—
3·0	60·0	—	11·5	15·3
4·0	—	—	30·0	20·0
5·0	—	—	60·0	34·5
6·0	—	—	enters top	65·0 (about)

FIG. 9.—Heights of Flame-caps in Coal-gas and in Water-gas.



Coal-gas in continuous lines, *water-gas* in dotted lines.
HH, cap heights over 10 mm. standard hydrogen flame.
H_1H_1, cap heights over hydrogen flame, reduced to 5 mm. in the presence of the gas.
H_2H_2, cap heights over 15 mm. hydrogen-flame.
OO, cap heights over oil flame, adjusted until the maximum cap is seen.

On comparing these cap-heights with those for the corresponding percentages of methane, it will be seen that they somewhat exceed the latter when the hydrogen flame is used, and are somewhat less than the methane cap-heights when the oil flame is employed. It is important to remember that this difference exists, if coal-gas is used to represent firedamp in the test-chamber.

Observation of Flame-caps in a Rapid Current of Air containing Coal-gas, and in the presence of Coal-dust.

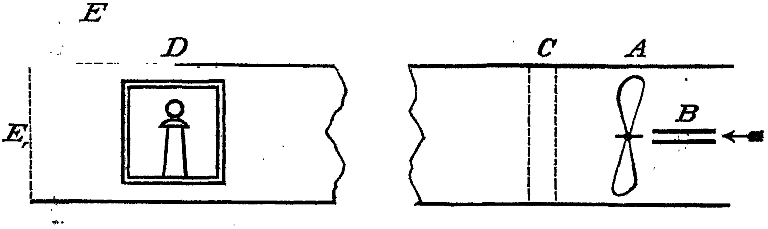
The test-chamber was originally introduced as a convenient form of apparatus for testing the delicacy of lamps. The chamber is far less costly, and less troublesome in use, and far more economical of the gases to be tested, than the forms of apparatus previously used for the same purpose.

But it has been frequently stated by practical mining men that the examination of the flame-caps in the still air of the test-chamber might lead to fallacious conclusions; since the "gas," when tested for in the mine, was contained in a rapidly moving air-current caused by the ventilating fan; and this movement of the air around the safety-lamp might entirely alter the character of the flame-caps which were seen with the same percentage of gas in still air.

That any effect could be produced on the cap by the movement of air seemed extremely unlikely, since no lamp is now considered safe in the mine the flame of which is at all affected by the ordinary ventilation current.

But whilst working with air containing coal-gas, which was easily obtainable in large quantity, the question was put to the test of experiment in the apparatus represented in fig. 10.

Fig. 10.



A square wooden tube, 12 feet in length and 14 inches square in section, had a fan, A, driven by a little electric motor, pushed into one end of it. This fan, when in rotation, drove an air-current at the rate of 300 feet per minute through the tube; a pipe, B, supplying coal-gas from the mains, fed the gas into the air current drawn by the fan.

The gas was thoroughly mixed with the air, partly by the action of the fan, and partly by the passage of the air current through coarse wire-gauze baffles, C. The lamp was introduced through a hinged window, D.

The hydrogen flame in the Ashworth's lamp was not affected in the least by this air-current. The cap shown over the flame indicated the presence of 0·5 per cent. of gas. At a given signal the hinged door E was dropped into position E_1, and closed the tube, and the fan was simultaneously stopped. The lamp was thus exposed to still air of the same composition as that which was previously in motion. Not the slightest effect upon the flame or cap was perceptible.

A similar experiment made with the reduced oil flame of the lamp, and with air containing a larger percentage of gas, showed that the air-current in motion produced no effect on the oil flame and its cap other than the quiescent air of the same composition did.

It may be concluded, therefore, that with a good safety-lamp the test-chamber indications are applicable to the conditions ordinarily found in the mine.

[*February* 7.—It was found, however, that the presence of fine coal-dust in very large proportion in the air rendered the test-flame so luminous that no cap could be seen even when gas was present. A very dusty atmosphere will render gas-tests impossible by all the forms of testing apparatus at present known. No interference arose from the dust ordinarily met with in the mine.]

Results of Flame-cap Measurements in Air containing Water-gas.

The poisonous nature of "water-gas," which is produced by passing steam over red-hot coke or charcoal, is well known. It is due to the large proportion of carbon monoxide present in the gas. Since water-gas is free from odour, it is very necessary to find, if possible, some delicate method of detecting it and of measuring its amount in air. Accordingly the heights of the flame-caps yielded by the hydrogen flame and by the oil flame of the safety-lamp, when the lamp was introduced into air containing known percentages of water-gas in the test-chamber, were measured. It was hoped that the flame-cap test might prove available for water-gas, as it is for firedamp and for coal-gas.

The samples of water-gas were prepared in the laboratory by passing steam over red-hot wood charcoal contained in a porcelain tube. The flame-indications proved that portions of the gas collected at different periods of the preparation were very different in composition; and a chemical analysis of the gas showed an undue proportion of carbon dioxide and a deficiency of carbon monoxide. The percent-

ages found were: carbon dioxide 18·5, carbon monoxide 5·4, hydrogen 76·1.

In this gas the oil flame showed no trustworthy indication until 4 per cent. of water-gas was present, when the height of the cap was 10·9 mm.; the cap rose to 15·3 mm. with 5 per cent. of water-gas. The cap over the standard hydrogen flame was 15·3 mm. when 0·25 per cent. of the gas was present, and rose gradually to 32 mm. when 5 per cent. was present. These results would indicate the flame-test to be of inferior delicacy for water-gas, but they require to be repeated with water-gas of better quality before final conclusions are drawn.

[*January* 27.—The flame-test, as applied to water-gas, was further investigated in the test-chamber, by employing an artificially prepared mixture of the separate gases. This mixture approximately corresponded to the average composition of water-gas. The percentages found on analysis were as follows:—

Hydrogen	49·6
Carbon monoxide	40·8
Carbon dioxide	2·6
Nitrogen	7

The complete series of flame-cap measurements are tabulated below and are graphically represented in fig. 9:—

Percentage of water-gas.	Hydrogen flame.			Colza-petroleum flame, maximum size.
	10 mm.	15 mm.	5 mm.	
0·25	10	25·3	Nil	Nil
0·5	14·5	33	"	"
1·0	17·2	..	"	"
2·0	28·7	..	"	8
3·0	31·4	..	"	8
4·0	*36	..	"	9
5·0	*40	..	11	14·5
6·0	*50·6	..	26	20

Percentages of carbon monoxide in the air less than 0·2 to 0·4, which is stated by the Prussian Firedamp Commission to be a poisonous proportion, are thus easily detectible by the hydrogen flame test in the safety-lamp.]

* In these measurements a delicate thread of flame, surmounting the conical cap, was not included.

The Measurement of the Percentage of Gas in Air by the Spiring of the full Luminous Flame.

It has recently been stated that the "spiring" of the full bright flame of the lamp is a very delicate and trustworthy test for gas. This effect occurs when the flame, which has been raised to the verge of smoking in air free from gas, is brought into air containing gas. The flame then stretches upwards a thin tongue, assumes a redder tinge, loses in brightness, and begins to smoke. It is stated that 0·5 per cent. of gas may thus be detected with ease and certainty. Experiments in the test-chamber failed to detect 0·5 per cent. of gas with an ordinary colza-petroleum flame. With a benzoline flame, however, 0·25 per cent. was detectible; and the spiring of the flame with 0·5 per cent. was most pronounced; while with 1 per cent. of gas a very distinct red spiring flame was produced which smoked freely. But it was at once evident that any safety-lamp flame when delicately adjusted for this test was very easily made to "spire" by causes other than the presence of firedamp; this was especially the case with the benzoline flame, which seemed to spire spontaneously at any moment.

When it is remembered that the "spiring" may be caused by any slight increase in the oil supply to the wick, or by the reduction of the supply of air or oxygen to the flame, arising from any cause other than the presence of "gas," it will be seen that the indications cannot be very trustworthy. The method is further impossible of application in many situations, since the flame must be adjusted for the test immediately before it is applied, and this must be done in air free from gas. Air known for a certainty to be free from "gas" is not obtainable in many situations where the test would have to be carried out. This test further clogs the meshes of the gauze with soot, and this impedes the proper action of the lamp.

The conclusion, drawn from a series of comparative experiments made in the test-chamber, was that the flame-cap test when carried out in the way already described, and with the apparatus mentioned above, exceeded both in delicacy and in trustworthiness the test depending upon the "spiring" of the full bright flame of the safety-lamp.

I have much pleasure in acknowledging the valuable and intelligent assistance rendered by W. T. Rigby in the above investigations.

II. "On a Meteoric Stone found at Makariwa, near Invercargill, New Zealand." By G. H. F. Ulrich, F.G.S., Professor of Mining and Mineralogy in the University of Dunedin, N.Z. Communicated by Professor J. W. Judd, F.R.S. Received December 14, 1892.

(Abstract.)

The specimen described in this memoir was found in the year 1879 in a bed of clay which was cut through in making a railway at Invercargill, near the southern end of the Middle Island of New Zealand. Originally this meteorite appears to have been about the size of a man's fist, and to have weighed 4 or 5 lbs., but it was broken up, and only a few small fragments have been preserved. The stone evidently consisted originally of an intimate admixture of metallic matter (nickel iron) and of stony material, but much of the metallic portion has undergone oxidation. Microscopic examination of thin sections shows that the stony portion, which is beautifully chondritic in structure, contains olivine, enstatite, a glass, and probably also magnetite, and through these stony materials the nickel iron and troilite are distributed. The specific gravity of portions of the stone was found to vary between 3·31 and 3·54, owing to the unequal distribution of the metallic particles. A partial chemical examination of this meteorite was made by the author and Mr. James Allen, but the complete analysis has been undertaken by Mr. L. Fletcher, F.R.S., of the British Museum. The analysis, which when finished will be communicated to this Society, has gone so far as to show that the percentage mineral composition of the Makariwa meteorite may be expressed approximately by the following numbers: nickel iron 1, oxides of nickel and iron 10, troilite 6, enstatite 39, olivine 44.

III. "On Operators in Physical Mathematics. Part I." By Oliver Heaviside, F.R.S. Received December 15, 1892.

Connexion between a Flux and a Force through an Operator.

1. In the investigation of physical questions we often have to answer such a question as this: Given a force f, a function of the time, acting at one place in a connected system, find the effect F, of some given type, produced by the force at its own or some other place. Or it may be that it is not an impressed force that is given, but displacement of some kind. Or, in order to produce mathe-

matical simplicity, we may have a space-distribution of force or of displacement given, whose effect is required.

To answer the question, we may investigate the general differential equation of the system, find its solution (series, integrals, &c.), and then introduce special values of constants or of functions to limit the generality of the problem, and bring the solution to satisfy the required conditions. Details may differ according to circumstances, but this may serve to describe the usual process.

2. There is, however, a somewhat different way of regarding the question. We may say that we have no special concern with the general solution which would express the disturbance anywhere due to initial energy throughout the system; but that we have simply a connected system, a given point (for example) of which is subjected to impressed force, communicating energy to the system, and we only want to know the effects due to this force itself. Since, therefore, the connexions are definite, we must have some definite connexion between the "flux" F and the "force" f, say

$$F = Yf, \tag{1}$$

where Y is a differentiating operator of some kind, a function of d/dt, the time-differentiator, for instance, when the connexions are of a linear nature. Here f is some given function of the time, and Y indicates the performance upon f of certain operations, whose result should be to produce the required function F.

3. An important point to be noted here is that there is, or should be, no indefiniteness about the above equation. The operator Y should be so determined as to fully eliminate all indeterminateness, and so that the equation contains in itself the full expression of the connexion between the force and the flux, without any auxiliary conditions, or subsequent limitations, except what may be implicitly involved in the equation itself.

Determinateness of a Solution through the Operator.

4. But as soon as we come to distinctly recognize this determinateness of connexion, another point of important significance presents itself. It should be possible to find F completely from f through the operator Y without ambiguity and without external assistance. That is to say, an equation of the form (1) not only expresses a problem, but also its solution. It may, indeed, not be immediately interpretable, but require conversion to some other form before its resultant meaning can be seen. But it is, for all that, a particular form of the solution, usually a condensed form, though sometimes it may be of far greater complexity than the full ordinary solution. In this respect the nature of the function f is of controlling importance.

We need not assert that the determinateness of F from equation (1) is true for all forms of the function Y that may be written down arbitrarily; but that it is true in the forms presenting themselves in dynamical problems seems to be necessitated.

5. We have, therefore, presented to us the problem of solving this equation for any particular form of Y that occurs. This may be very easy and obvious, or it may be excessively difficult and obscure. In the latter case it may be so merely because we do not know how to do it. Then we should find out. As our argument is that Y finds F from f definitely, there should be definite rules for the manipulation of the operator Y, or of the expression Yf, for its conversion to the form of an ordinary mathematical function, which will be the solution in the usual sense, freed from differentiating operations. We may find how to work by experiment. For, if two different methods lead to different results, one of which we find to be correct by independent tests, we can safely assert that one of the methods was partly wrong, whilst the other may have been wholly correct. So by practice we may come to know something about it.

6. Again, the function Y, regarded as an algebraical function, may admit of different forms of expression. These are algebraically equivalent, but to what extent they may be equivalent in their analytical aspects—for instance, one series involving differentiations equivalent to another involving integrations, and leading to results which are either identical or equivalent—cannot be safely said beforehand. It is, in its generality, a rather difficult and obscure matter. In special cases I find that forms of Y which are algebraically equivalent are also analytically equivalent; but I have not succeeded in determining the amount of latitude that is permissible in the purely algebraical treatment of operators. No doubt there are definite limitations, but they have to be found. I have, however, extensively employed the algebraical treatment experimentally,* subject to independent tests for guidance. It proved itself to be a powerful (if somewhat uncertain) kind of mathematical machinery. We may, for example, do in a line or two, work whose verification by ordinary methods may be very lengthy. On the other hand, the very reverse may be the case. I have, however, convinced myself that the subject is one that deserves to be thoroughly examined and elaborated by mathematicians, so that the method may be brought into general use in mathematical physics, not to supplant ordinary methods, but to supplement them; in short, to be used when it is found to be useful. As regards the theory of the subject, it is interesting in an unusual degree, and the interest is heightened by the mystery that envelops certain parts of it.

* The reader will find examples in my 'Electrical Papers,' vol. 2, of the treatment of irrational as well as rational operators.

Electromagnetic Operators.

7. Perhaps the best way of beginning the subject, to obtain a good idea of the nature of the operators and the advantages attending their use, is through the theory of a connected system of linear electrical conductors. For the electrical equations seem to be peculiarly fitted for the illustration of abstract dynamical properties in a clear manner, even when quite practical electromagnetic arrangements are concerned. We know that we may, by the application of Ohm's law to every conductor (or to circuits of conductors), express the steady current C_n in a conductor n due to an impressed force e_m in a conductor m by an equation

$$C_n = Y_{mn} e_m, \tag{2}$$

where Y_{mn} is some algebraical function of the resistances of the conductors—usually of all the resistances, although in special cases it may become independent of the values of some of them. Now, suppose it is not the steady current that is wanted, but the variable current when e_m varies. The answer is obviously given by the same equation when the function Y involves only resistances; that is, when there is no storage of electric or magnetic energy, so that Y is a constant not involving d/dt. Then the flux and the force keep pace together, and their ratio does not vary. It is, however, less obvious that the same equation should persist, in a generalized form, when every branch of the system is made to be any electromagnetic arrangement we please which would, in the absence of its connexions with the rest of the system, be a self-contained arrangement. To obtain the generalized form of Y_{mn} we have merely to substitute for the resistances concerned the equivalent resistance operators. That is, instead of $V = RC$, where V is voltage, C current, and R resistance, we have an equation $V = ZC$ in general for every conductor, where Z is the resistance operator appropriate to the nature of the conductor, which may be readily constructed from the electrical particulars. These Z's substituted in Y_{mn} in place of the R's make equation (2) fully express the new connexion between the flux C_n and the force e_m. There is much advantage in working with resistance operators because they combine and are manipulated like simple resistances. Of course (2) is really a differential equation, though not in the form usually given. To make it an ordinary differential equation we should clear of fractions, by performing such operations upon both sides of (2) as shall remove denominators and all inverse operations. It is then spread out horizontally to a great length (usually) and becomes very unmanageable. Also, we lose sight of the essential structure of the operator Y.

8. By arrangements of coils and condensers in our linear system

we may construct an infinite variety of resistance operators, and of conductance operators, such as Y_{mn} above. They are, however, always algebraical functions of p, and are finite. If expanded, equation (2) always becomes an ordinary linear equation of a finite number of terms. But if we allow conduction in masses, or dielectric displacement in masses (with allowance for propagation in time), the finite series we were previously concerned with become infinite series. This, at first, appears a complication, but it may be quite the reverse, for an infinite series following an easily recognized law may be more manageable than a finite series. Still, however, the equivalence to ordinary differential equations persists, provided our arrangement is bounded. But when we remove this restriction, and permit free dissipation of energy in space (or equivalently), another kind of operators comes into view. The complexity of the previous, due to the reaction of the boundaries, is removed; simpler forms of operators result, and they do not necessarily admit of the equations taking the form of ordinary differential equations, as they may be of an irrational nature. This brings us necessarily to the study of generalized differentiation, concerning which, more presently.

Operators admitting of Easy Treatment.

9. In the meantime, notice briefly some of the ideas and devices that occur generally in the treatment of operators. First of all, we may obtain the steady state of F due to steady f, when there can be a steady state of F, by simply putting $p = 0$ in the operator Y connecting them, p meaning d/dt. Even when there is no resultant steady state of the flux, as when reflections from a boundary continue for ever, the term $F = Y_0 f$ has its proper place and significance.

Next, we may notice that if the form of Y should involve nothing more than separate differentiations, as in

$$F = (a + bp + cp^2 + \dots) f, \tag{3}$$

then all we have to do is to execute the differentiations to obtain F from f. When f is a continuous function, this presents nothing special. When discontinuous, however, a special treatment may be needed.

In a similar manner, there may be only separate integrations or inverse differentiations indicated in Y, as when

$$F = (a + bp^{-1} + cp^{-2} + \dots) f. \tag{4}$$

Since f is a definite function of the time, so are its successive time-integrals. In this case, f may be discontinuous, and yet present no

difficulty. Suppose it is zero before and constant after the moment $t=0$. Then we shall have

$$p^{-1}f = ft, \text{ \&c.,} \qquad p^{-n}f = f\frac{t^n}{\underline{|n}}. \tag{5}$$

A combination of direct and inverse operations, which frequently occurs in the theory of waves, is exemplified in

$$F = \epsilon^{-pr/v}(a+bp^{-1}+cp^{-2}+\ldots\ldots)f. \tag{6}$$

Here we may perform the integrations first, getting the result $\phi(t)$ say, and then let the exponential operate, giving, by Taylor's theorem,

$$F = \phi(t-r/v). \tag{7}$$

Or we may let the exponential operator work first, and then perform the integrations. This may be less easy to manage, on account of the changed limits.

Two important fundamental cases, which constitute working formulæ, are

$$F = \frac{p}{p-a}f, \quad \text{and} \quad F = \frac{p}{p+a}f, \tag{8}$$

with unit operand, that is, $f=0$ before and constant after $t=0$. Here we may expand in inverse powers of p, getting, in the first case

$$F = (1+ap^{-1}+a^2p^{-2}+\ldots\ldots) = \epsilon^{at}, \tag{9}$$

and in the second case ϵ^{-at}. The latter expresses the effect of a unit impulse in a system having one degree of freedom, with friction, as when an impulsive voltage acts upon a coil.

Solutions for Simple Harmonic, Impulsive, and Continued Forces.

10. A very important case, admitting of simple treatment, occurs when the force is simple periodic, or a sinusoidal function of the time. It may happen that the resulting state of F is also sinusoidal. For this to occur, there must be dissipation of energy, to allow the initial departure from the simple periodic state to subside. We then have $p^2 = -n^2$ applied to F as well as f, where $n/2\pi$ is the frequency; so that the substitution of ni for p in Y brings equation (1) to the form

$$F = (Y_0+Y_1 i)f = (Y_0 + Y_1 n^{-1}p)f, \tag{10}$$

where Y_0 and Y_1 are functions of n^2. We now find F by a simple direct operation. This case is so important because its application

is so general, and its execution usually presents no difficulties, whilst the interpretation of the result may be valuable and instructive physically.

A continued constant force of unit strength, commencing when $t = 0$, may be represented by

$$p^0 = \left(\tfrac{1}{2} + \frac{1}{\pi}\int_0^\infty \frac{\sin nt}{n}\,dn\right), \tag{11}$$

using a well-known integral. We may apply this to equation (1), if desired, and obtain a particular form of solution. And from (11) we see that a unit impulse is represented by

$$p^1 = \frac{1}{\pi}\int_0^\infty \cos nt\,.\,dn \tag{12}$$

acting at the moment $t = 0$. This is, of course, the basis of Fourier's theorem. But, instead of the application of the fully developed Fourier's theorem, it is more convenient to use (12) itself. Thus, when f_0 is an impulse acting when $t = 0$, we have the equation

$$\mathrm{F} = \mathrm{Y}pf_0,$$

pf_0 representing the force. So, by (12),

$$\mathrm{F} = \frac{f_0}{\pi}\int_0^\infty \mathrm{Y}\cos nt\,.\,dn \tag{13}$$

gives us a particular form of the solution arising from an impulse. Take $p = ni$ in Y to convert the quantity to be integrated to an algebraical form.

Since a continuously varying force may be represented by a succession of infinitesimal impulses, we see that a single time-integration applied to (13), f_0 being then a function of the time, gives us a form of solution of the equation $\mathrm{F} = \mathrm{Y}f$, for any kind of f and Y that can occur. It is, however, a theoretical rather than a practical form of solution. For it usually happens that the definite integral is quite unamenable to evaluation. The same may be often said of the solution (13) for an impulse, and in such cases it may be questioned whether the form $\mathrm{F} = \mathrm{Y}f$ itself is not just as plain and intelligible. In fact, in certain cases, a very good way to solve (or evaluate) a solution in the form of a definite integral is to undo it, or convert it to the symbolical form $\mathrm{F} = \mathrm{Y}f$, and then solve it by any way that may be feasible. Nevertheless, it is interesting to know that we may have a full solution, and the definite integrals are sometimes practically workable, or may be transformed to easier kinds.

Partial Fractions and Normal Solutions.

11. There is also the method of partial fractions. It is not always applicable, and is especially inapplicable when the removal of boundaries drives the roots of the determinantal equation into contiguity. But the application is very wide, nevertheless. Put $Z = Y^{-1}$; then the solution of $F = Yf$, when f is constant, starting when $t = 0$, is

$$F = f\left\{\frac{1}{Z_0} + \Sigma\frac{\epsilon^{pt}}{p\,(dZ/dp)}\right\}, \tag{14}$$

where Z_0 is the steady Z, got by taking $p = 0$ in Z, and the summation ranges over the roots of the equation $Z = 0$, considered as an algebraical equation in p. That is, p is entirely algebraical in (14). Similarly, the effect of an impulse f_0 is represented by

$$F = f_0\,\Sigma\frac{\epsilon^{pt}}{dZ/dp}, \tag{15}$$

and from this again, by time-integration, we can obtain an expression for the effect due to any varying f, which may be quite as unmanageable as the previous definite integral for the same. On the other hand, (14) and (15) furnish the most direct and practical way of investigating certain kinds of problems, whether there be but a few or an infinite number of degrees of freedom. This method is the real foundation of all formulæ for the expansion of arbitrary functions in series of normal functions. For, find the impressed forcive that would keep up the arbitrary state. We may then apply the above to every element of the forcive to find its effect, and by integration throughout the system get the arbitrary functions expanded in normal functions. Or, without reference to impressed force, find the differential equation connecting any element of the initial state and the effect it produces later. It will be of a form similar to our $F = Yf$, and it may be similarly solved by a series, which contains the expression of the expansion of the initial state in the proper functions.

Or we may investigate the normal functions themselves, and employ their proper conjugate property to obtain the expansion representing any initial state. But this method does not apply very naturally to equations of the form we are considering.

Decomposition of an Operator into a Series of Wave Operators.

12. There is also another method which contrasts remarkably with the previous, viz., to decompose the operator Y into a series of other operators of a certain type expressing the propagation of waves. This is best illustrated by an example. Suppose the question is, given an

impressed force acting at one part of a long telegraph circuit, find the effect produced. One way would be to first find the effect due to a simple periodic force; from this the effect of an impulse follows; and from the latter the effect due to any f. A second way is by means of the normal functions, either through the conjugate property or by partial fractions. Lastly, we may decompose the operator Y into operators of the form which would exist were the circuit infinitely long, so that the effect of terminal reflections and absorptions does not appear. Say we have

$$F = (Y_0 + Y_1 + Y_2 + \ldots\ldots)f. \tag{16}$$

Then $F = Y_0 f$ will represent the initial wave from the source f, whilst the rest will express the succeeding reflected waves from the terminations of the circuit. The operators Y_0, &c., may be all of the same type, so that it suffices to solve $F = Y_0 f$, that is, convert it to an ordinary algebraic functional form, to obtain that form of the complete solution which has the greatest physical meaning, inasmuch as it shows in detail the whole march of F in terms of f. So does the solution in terms of normal functions, but not immediately, because the successive waves are expressed in the form of an infinite series of vibrating systems. Their resultant effect cannot be seen at once. We might, indeed, almost say that the form of solution in successive (or simultaneous) waves was *the* solution, being of the most explicit nature. Should, however, the impressed force be of a distributed nature, of the type suggested by a normal function, for example, then clearly it is the expression in terms of waves that becomes complex and unnatural. We also see that, although a direct transformation from one form of solution to another may be wholly impracticable algebraically, yet it may be readily carried out through the function Y as intermediary.

Treatment of an Irrational Operator. Solutions in Ascending Series.

13. The above general remarks are necessarily very sketchy. Some of the matters mentioned may be returned to, but the object of the preceding is merely to prepare the mind of the reader for the more transcendental matter to follow. Let us now consider how to treat irrational operators directly, without the assistance of definite integrals. The first form that presented itself to me was that exhibited by

$$Y = \left(\frac{K + Sp}{R + Lp}\right)^{\frac{1}{2}}, \tag{17}$$

where p is d/dt and R, S, K, L are constants. It occurs in the theory of a submarine cable or other telegraph circuit, and in other problems.

R and L are themselves differentiating operators of complicated form in general, or, more strictly, $R+Lp$ is a resistance operator, say R'', of very complex form. But it is quite sufficient to take the form $R+Lp$, where R is the effective resistance and L the inductance per unit length of circuit. S and K mean the permittance and leakage conductance per unit length.

Now we may readily obtain the simple periodic solution out of (17), by the before-mentioned substitution $p = ni$; and in doing so we may use the general operator R'', for that will then assume the form $R+Lp$. From this solution a wholly uninterpretable definite integral can be derived to express the effect of an impulse or of a steady impressed force. The question was, how to obtain a plain understandable solution from (17) itself to show the effect of a steady force. To illustrate, we may here take merely the case in which $K = 0$, whilst R and L are constants, because the inclusion of K (to be done later) considerably complicates the results. We have then to solve

$$F = \left(\frac{p}{a+p}\right)^{\frac{1}{2}}, \tag{18}$$

where a is a constant and $p = d/dt$. The operand is understood to be unity, that is, $f = 0$ before and $=1$ after $t = 0$. It is needless to write unit operands, and it facilitates the working to omit them. Now, the first obvious suggestion is to employ the binomial theorem to expand the operator. This may be done either in rising or in descending powers of p. Try first descending powers, since by experience with rational operators we know that that way works. We have

$$F = (1+ap^{-1})^{-\frac{1}{2}} = 1-\tfrac{1}{2}\frac{a}{p}+\frac{1\,.\,3}{2^2\lfloor 2}\frac{a^2}{p^2}-\frac{1\,.\,3\,.\,5}{2^3\lfloor 3}\frac{a^3}{p^3}+\dots\,. \tag{19}$$

The integrations, being separated from one another, can be immediately carried out through $p^{-n} = t^n/\lfloor n$, giving the result

$$F = 1-\frac{at}{2}+\frac{1\,.\,3}{(\lfloor 2)^2}\left(\frac{at}{2}\right)^2-\frac{1\,.\,3\,.\,5}{(\lfloor 3)^2}\left(\frac{at}{2}\right)^3+\dots, \tag{20}$$

or, which is the same,

$$F = \epsilon^{-\frac{at}{2}}\,I_0\left(\frac{at}{2}\right), \tag{21}$$

where I_0 is the well-known cylinder function. Now, that this result is correct may be tested independently, viz., by its correctly satisfying the differential equation concerned and the imposed conditions. We therefore obtain some confidence in the validity of the process employed.

In a precisely similar manner we may show that

$$\left(\frac{p}{p-a}\right)^{\frac{1}{2}} = \epsilon^{\frac{at}{2}} \mathrm{I}_0\left(\frac{at}{2}\right). \tag{22}$$

Further modifications are confirmatory. Thus, by making use of

$$f(p)\epsilon^{at} = \epsilon^{at} f(p+a), \tag{23}$$

we can shift ϵ^{at} and similar functions back and forth. Also using the stock formulæ

$$\frac{p}{p-a} = \epsilon^{at}, \qquad \frac{p}{p+a} = \epsilon^{-at}, \tag{23a}$$

we have the following transformations,

$$\left(\frac{p}{a+p}\right)^{\frac{1}{2}} = \epsilon^{-at}\,\epsilon^{at}\left(\frac{p}{a+p}\right)^{\frac{1}{2}} = \epsilon^{-at}\left(\frac{p-a}{p}\right)^{\frac{1}{2}}\epsilon^{at}$$

$$= \epsilon^{-at}\left(\frac{p-a}{p}\right)^{\frac{1}{2}}\left(\frac{p}{p-a}\right) = \epsilon^{-at}\left(\frac{p}{p-a}\right)^{\frac{1}{2}}. \tag{24}$$

In this we may use the result (22), and so come round to (21) again.

From the above we see that

$$\mathrm{I}_0(at) = \epsilon^{at}\left(\frac{p}{2a+p}\right)^{\frac{1}{2}} = \epsilon^{-at}\left(\frac{p}{p-2a}\right)^{\frac{1}{2}}; \tag{25}$$

and further, by shifting the exponentials to the right, to make them the operands (instead of t^0),

$$\mathrm{I}_0(at) = \left(\frac{p-a}{p+a}\right)^{\frac{1}{2}}\epsilon^{at} = \left(\frac{p+a}{p-a}\right)^{\frac{1}{2}}\epsilon^{-at}; \tag{26}$$

and now further again, by employing (23a) in place of the exponentials, we obtain

$$\mathrm{I}_0(at) = \left(\frac{p-a}{p+a}\right)^{\frac{1}{2}}\frac{p}{p-a} = \frac{p}{(p^2-a^2)^{\frac{1}{2}}}, \tag{27}$$

which is an entirely different kind of operator, since the square of p occurs under the radical sign, instead of the first power. But (27) may be readily tested and found to be not wanting. For expand by the binomial theorem, thus,

$$\frac{p}{(p^2-a^2)^{\frac{1}{2}}} = \left(1-\frac{a^2}{p^2}\right)^{-\frac{1}{2}} = 1+\tfrac{1}{2}\frac{a^2}{p^2}+\frac{1\,.\,3}{2^2\underline{|2}}\frac{a^4}{p^4}+\dots\,. \tag{28}$$

This may be immediately integrated, giving as result the series

$$\mathrm{I}_0(at) = 1+\frac{(at)^2}{2^2}+\frac{(at)^4}{2^2 4^2}+\dots\,, \tag{29}$$

the well-known formula for $I_0(at)$ in rising powers of the square of the variable, as required.

Transformation to a Descending Series.

14. There is such a perfect harmony in all the above transformations, without a single hitch, that you are tempted at first to think that you may do whatever you like with the operators in the way of algebraical transformation. There is a considerable amount of truth in this, but it is not wholly true. I shall show later some far more comprehensive and surprising transformations effected by simple means. At the same time I should emphasize the necessity of caution and of frequent verification, for no matter how sweetly the algebraical treatment of operators may work sometimes, it is subject at other times (owing to our ignorance) to the most flagrant failures.

But in the above we only utilized one way of effecting the binomial expansion. There is a second way, viz., in ascending powers of the differentiator. The two forms are algebraically equivalent so far as the convergency allows, but we have, so far, no reason to suppose that they are analytically equivalent. But on examination we find that they are. Thus, using the first of (25) and expanding, we get

$$I_0(at) = \epsilon^{at}\left(\frac{p}{2a+p}\right)^{\frac{1}{2}} = \epsilon^{at}\left(1+\frac{p}{2a}\right)^{-\frac{1}{2}}\left(\frac{p}{2a}\right)^{\frac{1}{2}}$$

$$= \epsilon^{at}\left(1-\frac{p}{4a}+\frac{1 . 3}{2}\left(\frac{p}{4a}\right)^2 - \dots\right)\left(\frac{p}{2a}\right)^{\frac{1}{2}}. \qquad (30)$$

Here the operand is t^0 or unity. Or we may make it $(p/2a)^{\frac{1}{2}}$ if we please. If we know its value, as a function of t, the rest of the work is easy, as it consists merely of differentiations. But nothing that has gone before gives any information as to the meaning of $p^{\frac{1}{2}}$, let alone its value. We may, however, find it indirectly. We may prove independently that when at is very big, $I_0(at)$ tends to be represented by $\epsilon^{at}(2\pi at)^{-\frac{1}{2}}$. From this we conclude that the value of $p^{\frac{1}{2}}$ must be $(\pi t)^{-\frac{1}{2}}$. Then (30) becomes

$$I_0(at) = \epsilon^{at}\left(1-\frac{p}{4a}+\frac{1 . 3}{2}\left(\frac{p}{4a}\right)^2 - \dots\right)\frac{1}{(2\pi at)^{\frac{1}{2}}}, \qquad (30a)$$

and now performing the rest of the differentiations, we arrive at

$$I_0(at) = \frac{\epsilon^{at}}{(2\pi at)^{\frac{1}{2}}}\left\{1+\frac{1}{8at}+\frac{1^2 3^2}{\lfloor 2(8at)^2}+\frac{1^2 3^2 5^2}{\lfloor 3(8at)^3}+\dots\right\}, \qquad (31)$$

which, on test, is found to be equivalent to the ascending series (29). Of course only the convergent part of the series can be utilized for

calculating the value of the function. It is, however, the series for practical use when at is big enough to make calculation by the convergent ascending series very lengthy. Stop when the convergency of (31) ceases. The result will be too big. Leave out the last counted term, and the result is too small. Counting only half the last convergent term, the result is nearly right, being a little too big. There seems no possible way of hitting the exact value. But still, when at is big, we can get quite close enough, to four or more figures, or any other number we please when at is sufficiently increased.

Fractional Differentiation.

15. Knowing in the above manner $p^{\frac{1}{2}}$, the values of $p^{\frac{3}{2}}$, $p^{\frac{5}{2}}$, &c., follow by complete differentiations. But although, on the basis of the above, a considerable amount of work may be done, and extensions made, yet it is desirable to stop for a moment. For the whole question of generalized differentiation is raised. The operator $p^{\frac{1}{2}}$ presents itself in analogous problems, along with $p^{\frac{1}{4}}$, &c. We want a general method of treating p^n, when n is not confined to be integral. Notice, however, in passing a remarkable peculiarity of the above investigation. If we had put $\mathrm{L} = 0$ in (17), as well as $\mathrm{K} = 0$, we should have had the form $\mathrm{Y} = p^{\frac{1}{2}}$ to consider at the beginning, with no evident means of treating it. By taking, on the other hand, a more general case, as we did, we avoided the fractional differentiation altogether, and easily obtained a convergent solution, viz., (21), through (18), (19), and (20). It is not always that we simplify by generalizing.

The sum total of the whole information contained in my mathematical library on the subject of generalized differentiation is contained in the remark made on p. 197 of the second part of Thomson and Tait's 'Natural Philosophy,' paragraph (n), relating to the process by which spherical harmonics of any degree may be derived from the reciprocal of a distance :—"The investigation of this generalized differentiation presents difficulties which are confined to the evaluation of P_s, and which have formed the subject of interesting mathematical investigations by Liouville, Gregory, Kelland, and others."

I was somewhat struck with this remark when I first read it, in trying to plough my way through the fertile though rather heavy field of Thomson and Tait, but as the subject was no sooner mentioned than it was dropped, it passed out of mind. Nor did the absence of any reference to the subject in other mathematical works, and in papers concerning mathematical physics generally, tend to preserve my recollection of the remark. Only when the subject was forced upon my attention in the above manner did I begin to investigate it, and not having access to the authorities quoted, I was compelled to work it out

myself. I cannot say that my results are quite the same, though there must, I think, be a general likeness. I can, however, say that it is a very interesting subject, and deserves to be treated in works on the Integral Calculus, not merely as a matter concerning differentiation, but because it casts light upon mathematical theory generally, even upon the elements thereof. And as regards the following brief sketch, however imperfect it may be, it has at least the recommendation of having been worked out in a mind uncontaminated by the prejudices engendered by prior knowledge acquired at second hand. I do not say it is the better for that, however.

Differentiation Generalized.

16. The question is, what is the meaning of ∇^n, if ∇ signify d/dx, when n has any value? This is, no doubt, partly a matter of convention; but apart from all conventions, there must be fundamental laws involved. Now observe that the effect of a whole differentiation ∇ upon the function x^n is to lower the degree by unity. This applies universally when n is not integral. When it is integral, there seem to be exceptions. But we can scarcely suppose that there is a real breach of continuity in the property. We also observe that a whole differentiation ∇ multiplies by the index, making $\nabla x^n = nx^{n-1}$; and again there are apparent exceptions. Now the first thing to do is to get rid of the exceptions. Next, the obvious conclusion from one ∇ lowering the index by unity, ∇^2 by two, and so on, is that ∇^n lowers the degree n times, whether n be integral or fractional. Further, since

$$\nabla \frac{x^n}{\underline{|n}} = \frac{x^{n-1}}{\underline{|n-1}}, \tag{32}$$

when n is positively integral, and $\underline{|n}$ is the factorial function $1.2.3\ldots\ldots n$; and, similarly,

$$\nabla^n \frac{x^n}{\underline{|n}} = 1, \tag{33}$$

whatever positive integer n may be, it is in agreement with the previous to define generalized differentiation by the last equation, for all values of n, provided we simultaneously define $\underline{|n}$ to be given by

$$\underline{|n} = n\underline{|n-1}, \tag{34}$$

for all values of n from $-\infty$ to $+\infty$, and to agree with the factorial function when n is integral, that is, $\underline{|1} = 1$, $\underline{|2} = 1.2$, $\underline{|3} = 1.2.3$, &c. We shall still call $\underline{|n}$ the factorial function, and $(\underline{|n})^{-1}$ the inverse factorial.

Now, by the above

$$\nabla \frac{x}{\underline{|1}} = \frac{x^0}{\underline{|0}} = 1, \tag{35}$$

if x be positive, as we shall suppose throughout. We conclude that $\underline{|0} = 1$.

Further differentiations give

$$\nabla x^0 = \frac{x^{-1}}{\underline{|-1}}, \qquad \nabla^2 x^2 = \frac{x^{-2}}{\underline{|-2}}, \quad \text{\&c.} \tag{36}$$

We, therefore, conclude that $(\underline{|-1})^{-1} = 0$, $(\underline{|-2})^{-1} = 0$, &c., or that the inverse factorial function vanishes for all integral negative values of n. We therefore know the value of the inverse factorial for all integral values of the variable, and a rough curve can be readily drawn. Say

$$y = \frac{1}{\underline{|n}}, \tag{37}$$

y being the ordinate, and n the abscissa. It has evidently a hump between $n = 0$ and $n = 1$, is positive for all + values of n, asymptotically tending to the n axis as n is increased, and is oscillatory on the other side of the origin. This is not demonstrative, but only highly probable so far.

The Inverse Factorial Function.

17. Now seek an algebraical function with equidistantly spaced roots on one side (either side) only of the origin. The function

$$(1-n)\left(1-\frac{n}{2}\right)\left(1-\frac{n}{3}\right)\dots\left(1-\frac{n}{r}\right) \tag{38}$$

vanishes at $n = 1, 2, 3$, &c., up to r. It has no other roots, and is positive when n is negative. Also, its value at $n = 0$ is 1. Similarly the function

$$(1+n)\left(1+\frac{n}{2}\right)\left(1+\frac{n}{3}\right)\dots\left(1+\frac{n}{r}\right) \tag{39}$$

vanishes at $n = -1, -2$, &c., up to $-r$; is unity at $n = 0$, and is positive when n is positive. These functions are identically the same as

$$1-n+\frac{n(n-1)}{\underline{|2}}-\frac{n(n-1)(n-2)}{\underline{|3}}+\dots\pm\frac{n(n-1)\dots(n-r+1)}{\underline{|r}} \tag{40}$$

and

$$1+n+\frac{n(n+1)}{\underline{|2}}+\frac{n(n+1)(n+2)}{\underline{|3}}+\dots+\frac{n(n+1)\dots(n+r-1)}{\underline{|r}} \tag{41},$$

where (40) corresponds to (38) and (41) to (39). At first sight, therefore, these functions might represent the inverse factorial, positive or negative, on making r infinite, for the value at the origin is correct, and the vanishing points are equidistantly spaced with unit step all the way to infinity on one side only of the origin. But something else happens when r is made infinite. The value of (38), by (40), becomes $(1-1)^n$, meaning the binomial expansion in rising powers of the second 1. It is, therefore, zero for *all* positive and infinity for all negative values of n. Similarly, (39) becomes $(1-1)^{-n}$, which is zero for all negative and infinity for all positive values of n. That is, from vanishing at detached points, the functions vanish all the way between them as well. Besides, apart from this, we cannot have the value of $\underline{|n}$ correct when n is integral.

We may, however, readily set the matter right. To get rid of the infinity on one side and vanishing all over on the other side of the origin, multiply the functions (38), (40) by r^n and (39), (41) by r^{-n}. Take

$$\frac{1}{\underline{|n}} = r^{-n}(1-1)^{-n} = r^{-n}(1+n)\left(1+\frac{n}{2}\right)\left(1+\frac{n}{3}\right)\ldots, \tag{42}$$

$$\frac{1}{\underline{|-n}} = r^{n}(1-1)^{n} = r^{n}(1-n)\left(1-\frac{n}{2}\right)\left(1-\frac{n}{3}\right)\ldots\ldots \tag{43}$$

We now satisfy all the requirements of the case, and when r is infinite make the inverse factorial curve (37) be a continuous curve from $-\infty$ to $+\infty$, subject to (34), in agreement with the known values when n is positively integral, and harmonizing with the generalized differentiation in (33).

Multiplying (42) and (43) together, we obtain

$$\frac{1}{\underline{|n}}\,\frac{1}{\underline{|-n}} = (1-n^2)\left(1-\frac{n^2}{4}\right)\left(1-\frac{n^2}{9}\right)\ldots = \frac{\sin n\pi}{n\pi}. \tag{44}$$

The multiplication therefore brings all the equidistant roots into play, on both sides of the origin.

This gives us the value of $\underline{|-n}$ in terms of $\underline{|n}$. Only the values of $\underline{|n}$ from $n=0$ to $n=1$ need be calculated, since (34) or (44) gives all the rest. But if we take $n=\frac{1}{2}$ in (44), we obtain

$$\underline{|\tfrac{1}{2}}\;\underline{|-\tfrac{1}{2}} = \tfrac{1}{2}(\underline{|-\tfrac{1}{2}})^2 = \tfrac{1}{2}\pi,$$

therefore
$$\underline{|-\tfrac{1}{2}} = \pi^{\frac{1}{2}}, \tag{45}$$

a fundamental result. We now know the value of $\underline{|n+\frac{1}{2}}$ when n is any integer, and this brings the matter down to the determination of $\underline{|n}$ from $n=0$ to $n=\frac{1}{2}$, for which a formula may be used.

Interpretation of Vanishing Differential Coefficients.

18. It should be noted that when we say that

$$\nabla^n \frac{x^n}{\underline{|n}} = 1, \tag{46}$$

for all values of n, the right member is really $x^0/\underline{|0}$, and means 0 on the left, and 1 on the right side of the origin of x. That is, it is the limiting form of the function $x^n/\underline{|n}$, when n is infinitely small *positive*. It is convenient in the treatment of equations of the form $F = Yf$ to have the function f zero up to a certain point, with consequently F also zero, and then begin to act. Similarly, the expression $\nabla x^0/\underline{|0}$, or $\nabla 1$ or $x^{-1}/\underline{|-1}$, although it has the value zero for all positive values of x, is infinite at the origin. But its total amount is finite, viz., 1. Imagine the unit amount of a quantity spread along an infinitely long line to become all massed at the origin. Its linear density will, in the limit, be represented by, as (12) is derived from (11),

$$\nabla 1 = \frac{1}{\pi}\int_0^\infty \cos mx \, dx. \tag{47}$$

It is zero except at $x = 0$. But its integral is still finite, being $\nabla^0 1$ or 1. If we draw the curve $y = x^n/\underline{|n}$, with n infinitely small, consisting of two straight lines, with a rounded corner, the curve derived from it by one differentiation will nearly represent the function $\nabla 1$, being nearly all heaped up close to the origin, and of integral amount 1. Similarly $\nabla^2 1$ means a double infinite point, $\nabla^3 1$ a triple infinite point, and so on. But it is the function $\nabla 1$ that is most useful in connexion with differentiating operations, whilst the others are less prominent.

But when n is taken to be infinitely small negative in $y = x^n/\underline{|n}$, then y drops from ∞ to 1 near the origin, or the corner is turned the other way. That is, the function x^n is unstable when n is zero. It is the difference of the curves $y = x^n$ with n infinitely small positive, and the same with n infinitely small negative, that makes the logarithmic function when infinitely magnified. But we should try to keep away from the logarithm in the algebraical treatment of operators.

Connexion between the Factorial and Gamma Functions.

18A. It will be seen by (42) that our factorial function is the gamma function of Euler somewhat modified and extended. Thus, when n is greater than -1 we have

$$\underline{|n} = \Gamma(n+1), \tag{48}$$

and this is also expressed by the definite integral

$$\int_0^\infty \frac{\epsilon^{-x}x^n}{\underline{|n}} = 1. \tag{49}$$

But when n is less than -1 we have the oscillatory curve of the inverse factorial, given by (44). We cannot use the definite integral to express $\underline{|n}$ when n is less than -1. The one-sided reckoning of the gamma function expressed in $\underline{|n} = \Gamma(n+1)$ is so exceedingly inconvenient in generalized differentiation that the factorial function had better be used constantly. For completeness and reference, we may add the general formula. Take the logarithm of (42) and arrange the terms suitably, and we obtain

$$\log \underline{|n} = -nC + \frac{n^2}{2}S_2 - \frac{n^3}{3}S_3 + \ldots., \tag{50}$$

where
$$S_m = 1 + \frac{1}{2^m} + \frac{1}{3^m} + \ldots., \tag{51}$$

and
$$C = S_1 - \log r = 0{\cdot}5772\,; \tag{52}$$

it being understood in (42) and (52) that r is made infinite.

From (50) we may obtain a series for the inverse factorial in rising powers of n. Thus,

$$\frac{1}{\underline{|n}} = 1 + Cn + (C^2 - S_2)\frac{n^2}{2} + (C^3 - 3CS_2 + 2S_3)\frac{n^3}{\underline{|3}}$$

$$+ (C^4 - 6C^2S_2 + 8CS_3 + 3S_2{}^2 - 6S_4)\frac{n^4}{\underline{|4}} + \ldots. \,. \tag{53}$$

As before remarked, only the value of $\underline{|n}$ from $n = 0$ to $\frac{1}{2}$ needs to be calculated. Any number of special formulæ for $\underline{|n}$ may be obtained from algebraical expansions involving this function.

A Suggested Cosine Split.

19. The above split of the function $(\sin n\pi)/n\pi$ into $1/\underline{|n}$ and $1/\underline{|-n}$, suggests other similar splits. In passing, one may be briefly noticed, the cosine split. Thus, take

$$f(n) = (r-\tfrac{1}{2})^{-n}\left(1+\frac{n}{\frac{1}{2}}\right)\left(1+\frac{n}{1\frac{1}{2}}\right)\left(1+\frac{n}{2\frac{1}{2}}\right)\ldots.\left(1+\frac{n}{r-\frac{1}{2}}\right), \tag{54}$$

and let $f(-n)$ be the same with the sign of n changed. Then, when $r = \infty$, we shall have

$$f(n)f(-n) = \cos \pi x. \tag{55}$$

By changing n to $n+1$ we find that

$$f(n+1) = f(n)\,\frac{2n+2r+1}{(2n+1)(r-\frac{1}{2})}\,, \tag{56}$$

so that when $r = \infty$,

$$f(n+1) = \frac{f(n)}{n+\frac{1}{2}}\,, \tag{57}$$

or

$$(n-\tfrac{1}{2})\,f(n) = f(n-1). \tag{58}$$

When $n = 0$, (54) gives $f(0) = 1$. Then (58) gives $f(n)$ for any integral n. Thus $f(1) = 2$, $f(2) = \frac{4}{3}$, $f(3) = \frac{8}{15}$, $f(4) = \frac{16}{3.5.7}$, &c. And on the negative side we have $f(-\frac{1}{2}) = 0$, $f(-1) = -\frac{1}{2}$, $f(-1\frac{1}{2}) = 0$, $f(-2) = \frac{3}{4}$, $f(-2\frac{1}{2}) = 0$, $f(-3) = -\frac{15}{8}$, &c. The curve is similar to that of the inverse factorial, but with a much bigger hump on the positive side, near $n = 1$. But I have not found any use for this cosine split, and we may now return to the other one.

The Exponential Theorem Generalized.

20. Although we cannot, owing to its limited applicability, use Euler's integral to express $\underline{|n}$ generally, we may employ it when found convenient, within its own range, and supplement the information it gives by other means. Thus, we know that

$$1 = \int_0^\infty \frac{\epsilon^{-x}x^n}{\underline{|n}}\,dx, \tag{59}$$

when n is over -1. Now the indefinite integral may be exhibited in two different ways, say

$$\int \frac{\epsilon^{-x}x^n}{\underline{|n}}\,dx = \epsilon^{-x}\left(\frac{x^{n+1}}{\underline{|n+1}} + \frac{x^{n+2}}{\underline{|n+2}} + \frac{x^{n+3}}{\underline{|n+3}} + \dots\right) = w_1, \tag{60}$$

in ascending powers of x multiplied by the exponential function, and by

$$\int \frac{c^{-x}x^n}{\underline{|n}}\,dx = -\epsilon^{-x}\left(\frac{x^n}{\underline{|n}} + \frac{x^{n-1}}{\underline{|n-1}} + \frac{x^{n-2}}{\underline{|n-2}} + \dots\right) = -w_2. \tag{61}$$

These are true for all values of n. Subtracting (61) from (60) we see that the function $w_1 + w_2$, or w say, must have the same value at any two finite limits we may choose for the integral. That is, the value of w is independent of the value of x.

Or we may proceed thus, and determine the value. Let n be greater than -1, and divide the integral (59) into two, one going

from 0 to x, the other from x to ∞. For the first use (60), since $w_1 = 0$ when $x = 0$; and for the second use (61), since $w_2 = 0$ when $x = \infty$. We then get, by (59),

$$1 = (w_1 + w_2), \tag{62}$$

or, which is the same,

$$\epsilon^x = \frac{x^n}{\underline{|n}} + \frac{x^{n+1}}{\underline{|n+1}} + \frac{x^{n+2}}{\underline{|n+2}} + \dots + \frac{x^{n-1}}{\underline{|n-1}} + \frac{x^{n-2}}{\underline{|n-2}} + \dots . \tag{63}$$

This is proved when n is greater than -1. But the change of n to $n-1$ in the series on the right of (63) makes no alteration. We therefore conclude that the series expresses ϵ^x for *all* values of n.

When $n = 0$ or any integer, positive or negative, we have the usual stopping series for ϵ^x. When n is fractional, we obtain semi-convergent series. Of course we obtain the whole series of forms by making n pass from 0 to 1. The most interesting case is that of $n = \frac{1}{2}$. This gives

$$\underline{|\tfrac{1}{2}} = x^{\frac{1}{2}}\epsilon^{-x}\Big(1 + \frac{2x}{3} + \frac{(2x)^2}{3.5} + \frac{(2x)^3}{3.5.7} + \dots$$
$$+ \frac{1}{2x} - \frac{1}{(2x)^2} + \frac{1.3}{(2x)^3} - \frac{1.3.5}{(2x)^4} + \dots\Big), \tag{64}$$

where the value of $\underline{|\tfrac{1}{2}}$ we know to be $\frac{1}{2}\underline{|-\tfrac{1}{2}} = \frac{1}{2}\pi^{\frac{1}{2}}$, by (45).

By means of this series we may pass from one to the other of the two forms of evaluation of Fresnel's integrals, due to Knochenhauer and to Cauchy respectively, which are given in works on Physical Optics.

21. The function called w_1 above we may obtain in a series of rising powers of x without the exponential factor in the following manner:—

$$w_1 = \nabla^{-1}\frac{\epsilon^{-x}x^n}{\underline{|n}} = \nabla^{-1}\epsilon^{-x}\nabla^{-n} = \nabla^{-1}(\nabla+1)^{-n}\epsilon^{-x}$$
$$= \nabla^{-1}(\nabla+1)^{-n}\frac{\nabla}{\nabla+1} = (\nabla+1)^{-(n+1)}, \tag{65}$$

which is immediately integrable by the binomial expansion; thus

$$w_1 = \nabla^{-(n+1)} - (n+1)\nabla^{-(n+2)} + \frac{(n+1)(n+2)}{\underline{|2}}\nabla^{-(n+3)} + \dots$$
$$= \frac{x^{n+1}}{\underline{|n+1}} - (n+1)\frac{x^{n+2}}{\underline{|n+2}} + \frac{(n+1)(n+2)}{\underline{|2}}\frac{x^{n+3}}{\underline{|n+3}} - \dots \tag{66}$$

To corroborate the method of getting (65) we may use (60). For this gives

$$w_1 = \epsilon^{-x}\nabla^{-(n+1)}(1+\nabla^{-1}+\nabla^{-2}+\dots) = \epsilon^{-x}\nabla^{-(n+1)}\epsilon^{x} = (\nabla+1)^{-(n+1)}, \quad (67)$$

again, as in (65). Now if we endeavour to express w_2 in a similar manner, we find that it will not work. But direct multiplication of the series in the brackets in (61) by the usual expansion of ϵ^{-x} gives

$$\begin{aligned} w_2 = {} & \frac{x^n}{\lfloor n}\left\{1-n+\frac{n(n-1)}{\lfloor 2}-\frac{n(n-1)(n-2)}{\lfloor 3}+\dots\right\} \\ & +\frac{x^{n-1}}{\lfloor n-1}\left\{1-(n-1)+\frac{(n-1)(n-2)}{\lfloor 2}-\frac{(n-1)(n-2)(n-3)}{\lfloor 3}+\dots\right\} \\ & +\dots \\ & +\frac{x^{n+1}}{\lfloor n+1}\left\{-(n+1)+\frac{(n+1)\cdot n}{\lfloor 2}-\frac{(n+1)\cdot n(n-1)}{\lfloor 3}+\dots\right\} \\ & +\frac{x^{n+2}}{\lfloor n+2}\left\{\frac{(n+2)(n+1)}{\lfloor 2}-\frac{(n+2)(n+1)n}{\lfloor 3}+\dots\right\}+\dots, \end{aligned} \quad (68)$$

or, which is the same,

$$\begin{aligned} w_2 = {} & \frac{x^n}{\lfloor n}(1-1)^n+\frac{x^{n-1}}{\lfloor n-1}(1-1)^{n-1}+\dots+\frac{x^{n+1}}{\lfloor n+1}(1-1)^{n+1}+\dots \\ & -\frac{x^{n+1}}{\lfloor n+1}+(n+1)\frac{x^{n+2}}{\lfloor n+2}-\frac{(n+1)(n+2)}{\lfloor 2}\frac{x^{n+3}}{\lfloor n+3}+\dots\,. \end{aligned} \quad (69)$$

Now the last line we know to express $-w_1$. Therefore, by (62), we get

$$1 = \frac{x^n}{\lfloor n}(1-1)^n+\frac{x^{n-1}}{\lfloor n-1}(1-1)^{n-1}+\dots+\frac{x^{n+1}}{\lfloor n+1}(1-1)^{n+1}+\dots, \quad (70)$$

and this is the result we shall obtain by multiplying the series on the right of (63) by the usual expansion of ϵ^{-x}. But (70) is only a special form of a more general formula that will appear later. We may use (44) to convert (70) to circular functions.

A Bessel Function Generalized.

22. The generalized expansion of ϵ^x may be at once applied to generalize other formulæ. Thus, we know that the solution of

$$\left(\nabla^2+\frac{1}{x}\nabla\right)u = u \quad (71)$$

in rising powers of x is

$$\mathrm{I}_0(x) = 1+\frac{x^2}{2^2}+\frac{x^4}{2^2 4^2}+\frac{x^6}{2^2 4^2 6^2}+\dots\,. \tag{72}$$

Put $x^2/4 = y$, and it becomes

$$\mathrm{I}_0(x) = 1+\frac{y}{(\lfloor 1)^2}+\frac{y^2}{(\lfloor 2)^2}+\frac{y^3}{(\lfloor 3)^2}+\dots\,. \tag{73}$$

Let the differentiator d/dy be called Δ. Then (73) may be at once written

$$\mathrm{I}_0(x) = 1+\frac{\Delta^{-1}}{\lfloor 1}+\frac{\Delta^{-2}}{\lfloor 2}+\frac{\Delta^{-3}}{\lfloor 3}+\dots = \epsilon^{\Delta^{-1}}. \tag{74}$$

There is nothing hypothetical about this. What is, however, entirely speculative in the absence of trial is whether it is legitimate to substitute the generalized exponential for the ordinary, and therefore generalized for complete differentiations. But on trial it will be found to work. Thus, using (63) with Δ^{-1} in place of x, we convert (74) to

$$u = \frac{\Delta^{-m}}{\lfloor m}+\frac{\Delta^{-(m+1)}}{\lfloor m+1}+\dots+\frac{\Delta^{-(m-1)}}{\lfloor m-1}+\dots, \tag{75}$$

writing u for the result; or, which means the same,

$$u = \frac{y^m}{(\lfloor m)^2}+\frac{y^{m+1}}{(\lfloor m+1)^2}+\dots+\frac{y^{m-1}}{(\lfloor m-1)^2}+\dots, \tag{76}$$

where, as before, $y = x^2/4$ in connexion with the Bessel function. Or

$$u = \frac{\left(\frac{x}{2}\right)^n}{\left(\lfloor\frac{n}{2}\right)^2}+\frac{\left(\frac{x}{2}\right)^{n+2}}{\left(\lfloor\frac{n+2}{2}\right)^2}+\dots+\frac{\left(\frac{x}{2}\right)^{n-2}}{\left(\lfloor\frac{n-2}{2}\right)^2}+\dots, \tag{77}$$

if $n = 2m$. We have $u_m - u_{m-1}$ for all values of m, and $u_0 = \mathrm{I}_0(x)$.

To test the validity when m is fractional, take $m = \frac{1}{2}$ or $n = 1$, then, by (77), we obtain

$$u = \frac{2}{\pi}\left(x+\frac{x^3}{3^2}+\frac{x^5}{3^2 5^2}+\frac{x^7}{3^2 5^2 7^2}+\dots+\frac{1}{x}+\frac{1^2}{x^3}+\frac{1^2 3^2}{x^5}+\frac{1^2 3^2 5^2}{x^7}+\dots\right). \tag{78}$$

This will by numerical calculation be found to give the same value as the series (72) for $\mathrm{I}_0(x)$. When x is as large as 10 the values agree to the fourth place by the convergent series in (78) alone, the semi-convergent series in (78) having a relatively small value. The value

I make to be 2815·71 by the ordinary formula, and 2815·75 by (78), including the semi-convergent part. But the last two figures are probably wrong, as there is a good deal of figuring involved in the calculation of both (72) and the convergent series in (78).

When smaller values of x are taken, the numerical agreement persists as far as the initial convergency of the descending series permits, as in the case of the series (31), for example. Later on I will coordinate (78) and (72) with the descending formula (31).

The companion formula to (78) is

$$v=\frac{2}{\pi}\left(\frac{1}{x}-\frac{1}{x^3}+\frac{1^2 3^2}{x^5}-\frac{1^2 3^2 5^2}{x^7}+\ldots\right)-\frac{2}{\pi}\left(x-\frac{x^3}{3^2}+\frac{x^5}{3^2 5^2}-\ldots\right). \quad (79)$$

We might expect this to be a form of the oscillatory function $J_0(x)$. But it is not. It represents the oscillatory companion to $J_0(x)$, say $G_0(x)$, which may be exhibited in an ascending series of the whole powers of x^2 together with a logarithm, so standardized as to vanish at infinity. This function will appear later. The double series (79) occurs in Lord Rayleigh's 'Sound.'* The series (78) I have not come across.

The Binomial Theorem Generalized.

23. Let us next generalize the binomial theorem in a similar manner. We have

$$\frac{(1+x)^n}{\underline{|n}}=\frac{1}{\underline{|n}}+\frac{x}{\underline{|1}\,\underline{|n-1}}+\frac{x^2}{\underline{|2}\,\underline{|n-2}}+\ldots \quad (80)$$

in ascending powers of x. Or

$$\frac{(1+x)^n}{\underline{|n}}=\frac{1}{\underline{|n}}+\frac{\nabla^{-1}}{\underline{|n-1}}+\frac{\nabla^{-2}}{\underline{|n-2}}+\ldots=\frac{1}{\nabla^n}\left(\frac{\nabla^n}{\underline{|n}}+\frac{\nabla^{n-1}}{\underline{|n-1}}+\ldots\right). \quad (81)$$

But in descending powers of x, which is the only other form generally known, we have

$$\frac{(x+1)^n}{\underline{|n}}=\frac{x^n}{\underline{|n}}+\frac{x^{n-1}}{\underline{|n-1}}+\frac{x^{n-2}}{\underline{|2}\,\underline{|n-2}}+\ldots$$

$$=\frac{1}{\nabla^n}\left(1+\nabla+\frac{\nabla^2}{\underline{|2}}+\frac{\nabla^3}{\underline{|3}}+\ldots\right)=\frac{\epsilon^{\nabla}}{\nabla^n}. \quad (82)$$

That we may use the generalized exponential we might infer from the two forms (80) and (82) being equivalent, combined with previous

* See p. 154, vol. 2. That (79) is $G_0(x)$ is not explicitly shown there, but it may be readily deduced.

experience relating to the Bessel function. Using it in (82) we find

$$\frac{(1+x)^n}{\underline{|n}} = \frac{1}{\nabla^n}\left(\frac{\nabla^m}{\underline{|m}}+\frac{\nabla^{m+1}}{\underline{|m+1}}+\dots+\frac{\nabla^{m-1}}{\underline{|m-1}}+\dots\right), \tag{83}$$

which by immediate integration gives

$$\frac{(1+x)^n}{\underline{|n}} = \frac{x^{n-m}}{\underline{|m}\,\underline{|n-m}}+\frac{x^{n-m+1}}{\underline{|m-1}\,\underline{|n-m+1}}+\dots+\frac{x^{n-m-1}}{\underline{|m+1}\,\underline{|n-m-1}}+\dots, \tag{84}$$

for all values of m.

The case $m=0$ or any integer is that of (82), and $m=n$ is that of (80). The whole series of forms ranges between $m=0$ and $m=1$, because they recur. When $m=\frac{1}{2}$ we have

$$\frac{(1+x)^n}{\underline{|n}} = \frac{x^{n-\frac{1}{2}}}{\underline{|\tfrac{1}{2}}\,\underline{|n-\tfrac{1}{2}}}+\frac{x^{n+\frac{1}{2}}}{\underline{|-\tfrac{1}{2}}\,\underline{|n+\tfrac{1}{2}}}+\dots+\frac{x^{n-\frac{3}{2}}}{\underline{|\tfrac{3}{2}}\,\underline{|n-\tfrac{3}{2}}}+\dots\;. \tag{85}$$

If in this we take $n=1$, we find that the terms can be arranged in pairs, thus

$$1+x = \frac{x^{\frac{1}{2}}}{\underline{|\tfrac{1}{2}}\,\underline{|\tfrac{1}{2}}}+\frac{x^{\frac{3}{2}}+x^{-\frac{1}{2}}}{\underline{|-\tfrac{1}{2}}\,\underline{|\tfrac{3}{2}}}+\frac{x^{\frac{5}{2}}+x^{-\frac{3}{2}}}{\underline{|-1\tfrac{1}{2}}\,\underline{|2\tfrac{1}{2}}}+\dots, \tag{86}$$

or, which is the same,

$$\frac{\pi}{4}(1+x) = x^{\frac{1}{2}}\left(1+\frac{x+x^{-1}}{1\,.\,3}-\frac{x^2+x^{-2}}{3\,.\,5}+\frac{x^3+x^{-3}}{5\,.\,7}-\dots\right). \tag{87}$$

The best value of x is obviously 1.

When two variables x and y are used in the binomial theorem, we have, using ∇ for d/dx and Δ for d/dy,

$$\frac{(y+x)^n}{\underline{|n}} = \frac{x^n}{\underline{|n}}+\frac{x^{n-1}}{\underline{|n-1}}\,y+\frac{x^{n-2}}{\underline{|n-2}}\,\frac{y^2}{\underline{|2}}+\dots = \nabla^{-n}+\nabla^{-(n-1)}\Delta+\dots$$

$$= \frac{\nabla^{-n}}{1+\nabla/\Delta} = \frac{\Delta^{-n}}{1+\Delta/\nabla} = \nabla^{-n}\epsilon^{y\nabla} = \Delta^{-n}\epsilon^{x\Delta} \tag{88}$$

We may use any of these forms. Selecting the last but one, and using the generalized exponential, we have

$$\epsilon^{y\nabla}\nabla^{-n} = \nabla^{-n}\left\{\frac{(y\nabla)^m}{\underline{|m}}+\frac{(y\nabla)^{m-1}}{\underline{|m-1}}+\dots+\frac{(y\nabla)^{m+1}}{\underline{|m+1}}+\dots\right\}; \tag{89}$$

therefore

$$\frac{(x+y)^n}{\underline{|n}}=\frac{y^m x^{n-m}}{\underline{|m}\;\underline{|n-m}}+\frac{y^{m-1}x^{n-m+1}}{\underline{|m-1}\;\underline{|n-m+1}}+\dots+\frac{y^{m+1}}{\underline{|m+1}}\,\frac{x^{n-m-1}}{\underline{|n-m-1}}+\dots, \tag{90}$$

where m may have any value, and x, y may be exchanged.

Taylor's Theorem Generalized.

24. We may also apply the generalized exponential to Taylor's theorem for the expansion of a function in powers of the variable. For this theorem is expressed by

$$f(x+h)=\epsilon^{h\nabla}f(x), \tag{91}$$

and, if this be true generally, irrespective of the wholeness of the differentiations, we must have

$$f(x+h)=\left\{\frac{h^n}{\underline{|n}}\nabla^n+\frac{h^{n+1}}{\underline{|n+1}}\nabla^{n+1}+\dots+\frac{h^{n-1}}{\underline{|n-1}}\nabla^{n-1}+\dots\right\}f(x). \tag{92}$$

Whether this is true for any function $f(x)$, with the usual limitations, I cannot say. There are probably other necessary limitations.

As examples, take $f(x)=1$. Then we obtain

$$1=\frac{h^n x^{-n}}{\underline{|n}\;\underline{|-n}}+\frac{h^{n-1}x^{1-n}}{\underline{|n-1}\;\underline{|1-n}}+\dots+\frac{h^{n+1}x^{-n-1}}{\underline{|n+1}\;\underline{|-n-1}}+\dots. \tag{93}$$

Here put $c=h/x$; then, by using (44), we have the result

$$1=c^n\frac{\sin n\pi}{n\pi}+c^{n+1}\frac{\sin(n+1)\pi}{(n+1)\pi}+\dots+c^{n-1}\frac{\sin(n-1)\pi}{(n-1)\pi}+\dots, \tag{94}$$

where c is to be positive. When $n=\frac{1}{2}$, this reduces to

$$\frac{\pi}{2}=a+\frac{1}{a}-\tfrac{1}{3}\left(a^3+\frac{1}{a^3}\right)+\tfrac{1}{5}\left(a^5+\frac{1}{a^5}\right)-\dots, \tag{95}$$

where a is written for $c^{\frac{1}{2}}$. It is obviously right when $a=1$.

The formula (70) may be derived from (94) by the use of (44).

Special Formulæ for Factorials.

25. The binomial generalization before given is, of course, a special case of (92), namely, $f(x)=x^n/\underline{|n}$. It will be observed that the series

it gives may be convergent. Thus we may obtain convergent special formulæ for $\underline{|n}$. Thus, take $m = \frac{1}{4}$, $n = \frac{1}{2}$ in (84). We obtain

$$\frac{(1+x)^{\frac{1}{2}}}{\underline{|\frac{1}{2}}} = \frac{x^{\frac{1}{4}}}{(\underline{|\frac{1}{4}})^2}\left\{1+\frac{x+x^{-1}}{5}-\frac{3(x^2+x^{-2})}{5.9}+\frac{3.7(x^3+x^{-3})}{5.9.13}-\dots\right\}, \quad (96)$$

and when $x = 1$, we have the series

$$\left(\frac{8}{\pi}\right)^{\frac{1}{2}}(\underline{|\tfrac{1}{4}})^2 = 1+\tfrac{2}{5}(1-\tfrac{3}{9}(1-\tfrac{7}{13}(1-\tfrac{11}{17}(1-\dots \quad (97)$$

Similarly, $m = \frac{2}{3}$, $n = \frac{1}{3}$, $x = 1$, gives

$$\frac{8\pi^2}{81.4^{\frac{1}{3}}(\underline{|\frac{1}{3}})^3} = 1-\tfrac{1}{5}(1-\tfrac{4}{8}(1-\tfrac{7}{11}(1-\tfrac{10}{14}(1-\tfrac{13}{17}(1-\dots; \quad (98)$$

and so on.

Property of the Generalized Exponential.

26. Notice that the operation ∇^m performed upon the generalized ϵ^x reproduces it when m is integral, but gives an equivalent series when m is fractional. If, then, we take the special form of the ordinary stopping series for ∇^m to work upon, we require to imagine that the zero terms are in their places, thus,

$$\epsilon^x = \dots + \frac{x^{-2}}{\underline{|-2}} + \frac{x^{-1}}{\underline{|-1}} + 1 + x + \frac{x^2}{\underline{|2}} + \dots \quad (99)$$

All terms before the 1 are zero, but not their rates of variation with x in the generalized sense, if we are to have harmony with the behaviour of the general form of ϵ^x. This is transcendental: and there is much that is transcendental in mathematics.

The above generalizations are somewhat on one side of our subject of the treatment of operators, though suggested thereby. I propose to continue the main subject in a second paper.

IV. "On Certain Ternary Alloys. Part VII. Alloys containing Zinc, together with Lead (or Bismuth) and Cadmium (or Antimony)." By C. R. Alder Wright, D.Sc., F.R.S., Lecturer on Chemistry and Physics in St. Mary's Hospital Medical School. Received January 5, 1893.

In the previous six papers* it has been shown that when silver is substituted for tin as "solvent" metal the critical curve deduced is uniformly *raised*, whether the two immiscible metals employed be lead and zinc, bismuth and zinc, lead and aluminium, or bismuth and aluminium; it was thought desirable to examine similarly the effects of other solvent metals, more especially *cadmium* and *antimony*, direct experiments having shown that these metals respectively are miscible in all proportions *whilst molten* with either lead, bismuth, or zinc; although in some cases more or less marked segregation of the metals from one another takes place on cooling so that solidification commences. Accordingly the experiments described below were made with zinc as lighter immiscible metal; but on substituting aluminium for zinc, so as to prepare ternary alloys containing lead (or bismuth), aluminium, and cadmium (or antimony), it was found that the close analogy between zinc and aluminium found in Part VI to subsist in alloys containing one or other of these metals along with lead (or bismuth) and tin (or silver) breaks down with these other combinations; in the case of the alloys where cadmium is the solvent metal, because molten cadmium and aluminium (contrary to the usual statements in the text-books) are *not* completely miscible together, like zinc and aluminium, but behave as lead and aluminium, or bismuth and aluminium, the heavier metal dissolving only a few tenths per cent. of aluminium, whilst this latter dissolves only some 2 or 3 per cents. of the other metal; and in the case of the alloys where antimony is the solvent, because aluminium and antimony combine together to form a most remarkable compound, represented by the formula AlSb,† which possesses a melting point higher by upwards of 340° C. than that of the least fusible of its constituents; thus, whilst antimony melts at about 432° C., and aluminium at below 700° C. (varying somewhat according to its purity or otherwise), the compound AlSb appears (from observations kindly made for the author by Professor Roberts-Austen with the Le Chatelier pyrometer) to have a solidifying point close to that of gold, viz., 1045°. The effect of the

* Part I, 'Roy. Soc. Proc.,' vol. 45, p. 461; Part II, vol. 48, p. 25; Part III, vol. 49, p. 156; Part IV, vol. 49, p. 174; Part V, vol. 50, p. 372; Part VI, vol. 52, p. 11.

† 'Journal Society of Chemical Industry,' 1892, p. 492.

tendency towards the formation of this difficultly fusible compound with alloys richer in antimony is to cause a partial separation of antimony and aluminium thus combined together in the form of solid particles when the melted alloy is allowed to stand for some time at temperatures not exceeding some 900°; which separation interferes with the accurate tracing out of the upper portions of the critical curve.

The discussion in detail of the results obtained with ternary alloys, containing simultaneously aluminium and cadmium (or antimony) is postponed to a future communication; but it may be here noticed that ternary alloys, such as aluminium, cadmium, and lead, or aluminium, cadmium, and bismuth, belong to a class different from that of the ternary alloys hitherto examined. Calling the three constituents A, B, and C respectively, three pairs of metals may be formed, viz., AB, AC, and BC. In the alloys previously described, *one only* of these pairs consists of two metals not miscible together in all proportions, *e.g.*, in the case of lead, zinc, and tin, the pair lead and zinc are immiscible; whilst the pairs lead and tin, zinc and tin are perfectly miscible. With alloys of the aluminium-cadmium-lead class, on the other hand, *two* pairs of immiscible metals exist, *e.g.*, aluminium and cadmium, and aluminium and lead. The effect of this difference is to modify very largely the nature of the critical curve deducible by means of the triangular method of graphical representation.

A priori, there seems no reason why a ternary alloy could not exist of a third class, where *all three* pairs of metal are immiscible; hitherto, however, the author has not met with such a case in actual practice. If mixed in approximately equal quantities of the three constituents, such an alloy should divide itself into *three* different ternary alloys, forming three different layers, viz., one consisting of A, with a small admixture of B and C; another chiefly containing B, with a little A and C; and a third principally consisting of C, together with a little A and B. Although metallic mixtures of this class have not been obtained so far, other sets of three liquids have been found possessing this peculiar physical character; thus a mixture of *water*, *castor oil* (genuine—not largely adulterated with other kinds of fixed oil), and *petroleum hydrocarbons* (such as ordinary kerosine) separates into three layers when well shaken up and then allowed to stand.

Alloys containing Zinc with Lead (or Bismuth) and Cadmium as Solvent.

The experiments were carried out with the lead bath arrangement in precisely the same way as before, the weighed metals being fused together with a little cyanide of potassium, and well intermixed,

poured into hot clay test-tubes, and kept 7—8 hours in the lead bath. To diminish possible volatilisation of cadmium, the temperature employed was somewhat lowered, so that it always lay between 550° and 650°, averaging near to 600°.

The analysis of the alloys containing lead, zinc, and cadmium was effected by dissolving in nitric acid and evaporating with sulphuric acid; the filtrate from the lead sulphate was diluted, further acidulated with hydrochloric acid, and treated with sulphuretted hydrogen till all cadmium was precipitated. The cadmium sulphide thus thrown down generally carried down more or less zinc sulphide; to separate this, the mixed sulphides were dissolved in a little hot concentrated hydrochloric acid, diluted, and again treated with sulphuretted hydrogen, the process being repeated when necessary, till no more zinc was contained in the filtrate. The cadmium sulphide finally obtained was dissolved in hydrochloric acid, and bromine water added in excess to destroy sulphuretted hydrogen; finally, the cadmium was precipitated boiling by sodium carbonate, and ultimately weighed as CdO. The acid liquors containing zinc were united and treated with ammonia and ammonium sulphide, the zinc sulphide being finally converted into carbonate and weighed as ZnO, correction being made for traces of Fe_2O_3 when present.

The alloys containing bismuth instead of lead were examined in the same way, excepting that the alloy was dissolved in nitro-hydrochloric acid, and the solution evaporated to dryness and treated with a large bulk of water, so as to separate the bismuth as oxychloride. This oxychloride was boiled with ammonium sulphide to remove chlorine (which is apt to interfere with proper conversion into carbonate), and the resulting bismuth sulphide dissolved in nitric acid, the solution being precipitated boiling with ammonia and ammonium carbonate, and the bismuth finally weighed as Bi_2O_3.

Mixtures of Lead, Zinc, and Cadmium.

The following figures were obtained as the averages from the examination of 24 compound ingots (48 alloys). In the mixtures used for deducing the earlier ties, the lead and zinc were used in about equal quantities; subsequently the proportion of lead was increased relatively to the zinc, until finally the two metals were employed in about the ratio 10 to 1, this being found necessary to bring about the formation of the lighter and heavier alloys in not widely different quantities. The percentages are uniformly reckoned on the sum of the weights of the three metals found as 100.

No. of tie line.	Heavier alloy.			Lighter alloy.			Excess of cadmium percentage in lighter alloy over that in heavier.
	Cadmium.	Lead.	Zinc.	Cadmium.	Lead.	Zinc.	
0	0	98·76	1·24	0	1·14	98·86	0
1	5·17	92·85	1·98	10·12	1·54	88·34	4·95
2	13·45	83·89	2·66	24·91	2·44	72·65	11·46
3	17·57	80·18	2·25	31·48	2·85	65·67	13·91
4	19·14	78·73	2·13	36·50	3·49	60·01	17·36
5	21·44	76·34	2·22	46·42	5·96	47·62	24·98
6	22·68	75·13	2·19	55·72	10·16	34·12	33·04
7	22·23	76·32	1·45	59·20	13·44	27·36	36·97
8	22·37	76·31	1·32	70·78	18·17	11·05	48·41
9	23·70	74·85	1·45	72 76	18·80	8·44	49·06
10	33·80	64·35	1·85	72·06	21·22	6·72	38·26
11	46·93	50·82	2·25	69·50	25·86	4·64	22·57
12	50·30	47·45	2·25	67·67	28·29	4·04	17·37

Fig. 1 represents these values plotted on the triangular system, the internal dotted line being the curve obtained for the temperature 650°, with tin as solvent metal (Part V). The position of the limiting point L deduced by Stokes' second method is that where $A+A' = 73·0$, and $B+B' = 5·8$; whence—

Lead	36·5
Zinc	2·9
Cadmium	60·6
	100·0

This corresponds with a ratio between lead and zinc not far from that indicated by the formula Pb_4Zn, widely different from the corresponding ratios found when tin and silver were the solvent metals, respectively close to $PbZn_6$ and Pb_2Zn,

The six tie lines, Nos. 4 to 9, all spring from points very close together on the left-hand branch of the curve, although the respective conjugate points on the right-hand branch are widely divergent. The centre of this cluster of points is near the point P, giving the values

Cadmium	22
Lead	76
Zinc	2
	100

which figures represent a ratio between cadmium and lead close to that indicated by the formula Pb_2Cd.

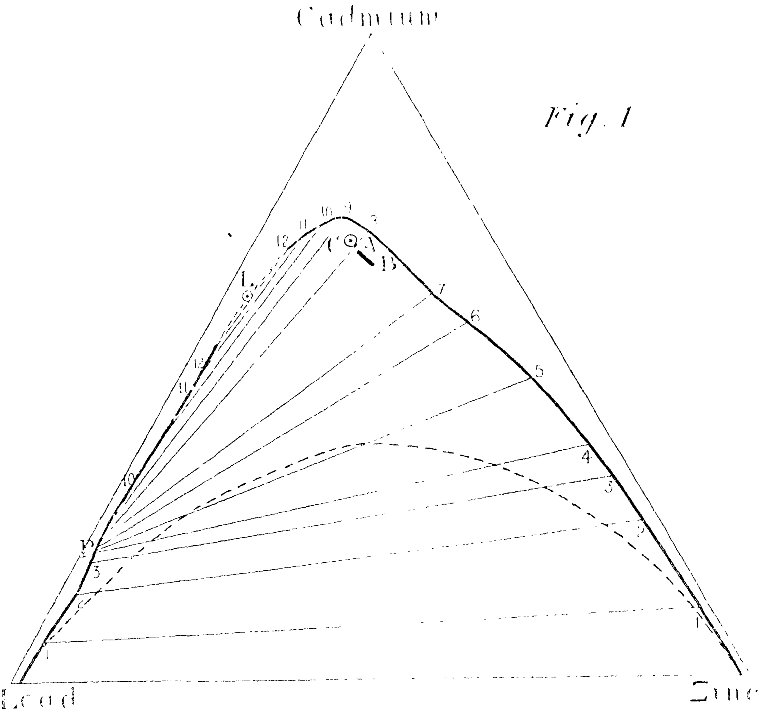


Fig. 1

	Calculated.	Found.		
Cd..........	21·3	22	=	22·4
Pb_2	78·7	76	=	77·6
	100·0	98		100·0

Obviously, this configuration strongly suggests that a definite atomic compound Pb_2Cd exists, the tendency towards the formation of which is the cause of the convergence together of the tie lines; as in the analogous case with lead-zinc-tin alloys (Part V), where a similar convergence suggests the existence of the definite compound $SnZn_4$.

Some observations were made at temperatures a little higher than those employed in the foregoing experiments, the result of which was to indicate that the critical curve is somewhat rapidly lowered in position with a raised temperature, at any rate, as regards its uppermost portion. Thus, at temperatures ranging between 600° and 700°, and averaging about 650°, or some 50° higher than before, it was found that no separation at all took place with a mixture containing

Cadmium	67·8
Lead	19·6
Zinc	12·6
	100·0

This mixture is represented by the point C in Fig. 1, obviously lying distinctly *inside* the critical curve for about 600°, although necessarily *outside* that for about 650°.

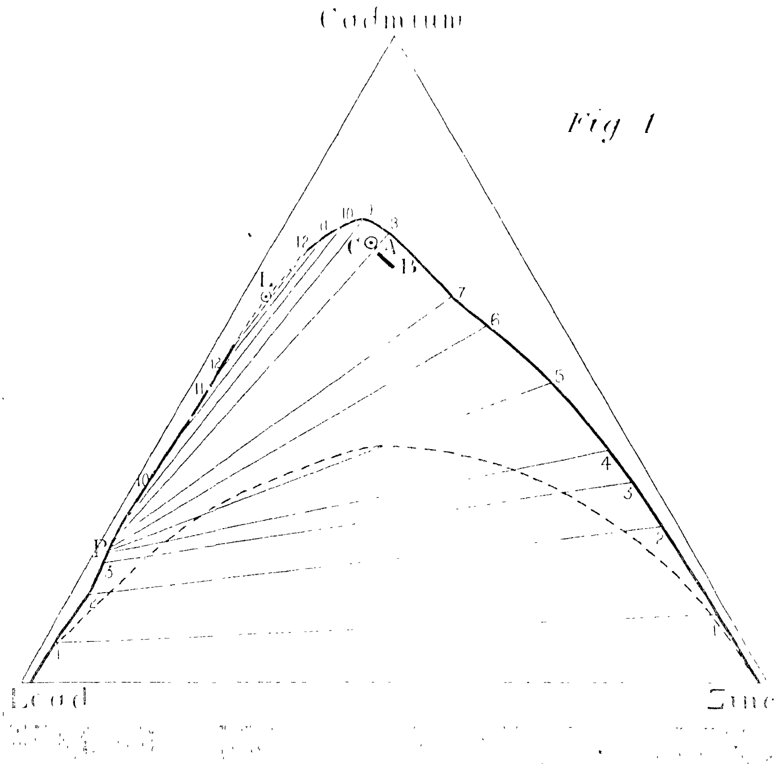
Similarly, four mixtures that did separate at 650° gave points belonging to a curve lying well inside that obtained as above for about 600°; averaging the results in pairs as usual, the following values were obtained :—

Heavier alloy.			Lighter alloy.		
Cadmium.	Lead.	Zinc.	Cadmium.	Lead.	Zinc.
22·11 22·72	76·44 74·98	1·45 2·30	63·96 67·35	19·59 18·85	16·45 13·80

The line AB represents the portion of the right-hand branch of the critical curve thus traced out at about 650°, obviously lying well inside that found at near 600°; the corresponding points on the left-hand branch lie close to the point P, approximately corresponding with the ratio Pb_2Cd.

Mixtures of Bismuth, Zinc, and Cadmium.

A series of twenty compound ingots (forty alloys) gave the following average results; in the earlier cases the bismuth and zinc were employed in equal quantities; in the later ones the proportion of zinc was increased up to 1½ times the bismuth:—



	Calculated.	Found.		
Cd..........	21·3	22	=	22·4
Pb_2	78·7	76	=	77·6
	100·0	98		100·0

Obviously, this configuration strongly suggests that a definite atomic compound Pb_2Cd exists, the tendency towards the formation of which is the cause of the convergence together of the tie lines; as in the analogous case with lead-zinc-tin alloys (Part V), where a similar convergence suggests the existence of the definite compound $SnZn_4$.

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No. of tie line.	Heavier alloy.			Lighter alloy.			Excess of cadmium percentage in lighter alloy over that in heavier.
	Cadmium.	Bismuth.	Zinc.	Cadmium.	Bismuth.	Zinc.	
0	0	85·72	14·28	0	2·32	97·68	0
1	6·98	78·84	14·18	3·77	3·32	92·91	− 3·21
2	14·27	71·34	14·39	9·80	4·46	85·74	− 4·47
3	21·19	64·38	14·43	14·37	5·54	80·09	− 6·82
4	28·24	56·75	15·01	18·04	6·02	75·94	−10·20
5	33·67	50·34	15·99	24·48	6·34	69·18	− 9·19
6	38·06	46·02	15·92	29·32	6·90	63·78	− 8·74
7	46·20	37·08	16·72	37·42	7·01	55·57	− 8·78
8	53·88	24·93	21·19	46·73	9·07	44·20	− 7·15
9	54·48	21·21	24·31	49·73	12·29	37·98	− 4·75
10	53·08	21·55	25·37	50·93	15·25	33·82	− 2·15

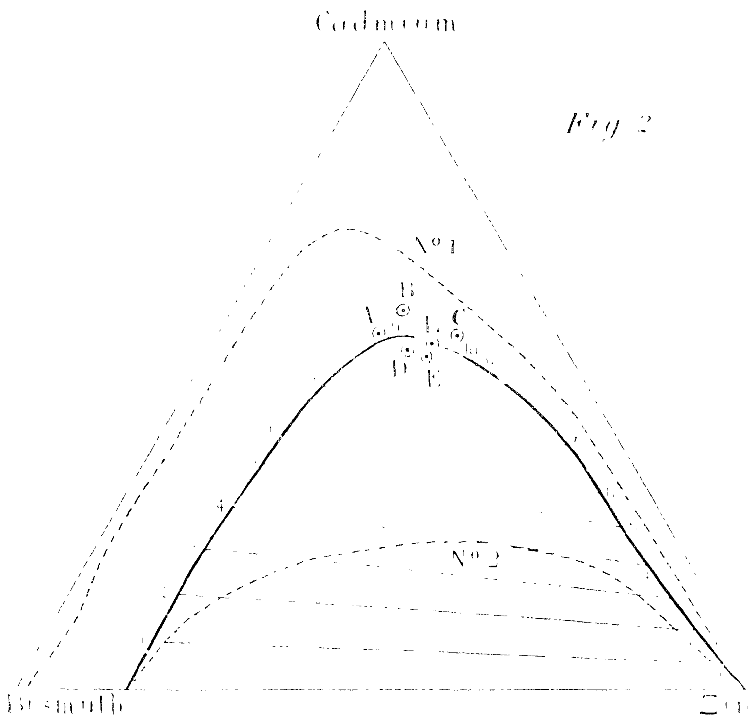


Fig. 2 represents these values plotted on the triangular system; the outer dotted line (No. 1) being the curve above described obtained

with lead, zinc, and cadmium, and the inner one (No. 2) that obtained at 650° with bismuth, zinc, and tin (Part V). The position of the limiting point L is deduced by Stokes's 2nd method, as that where $A+A' = 39$ and $B+B' = 57$, whence

Bismuth..............	19·5
Zinc	28·5
Cadmium	52·0
	100·0

This corresponds with a ratio between bismuth and zinc close to that indicated by the formula $BiZn_5$; the corresponding ratios previously found with tin and silver as solvent metals being respectively close to $BiZn_{10}$ and $BiZn_2$.

The three points marked A, B, C, lying *outside* the critical curve, represent the compositions of three mixtures that did not separate at temperatures lying near to 600°, the mean temperature corresponding with the critical curve delineated; on the other hand, the points D and E, lying *inside* the critical curve, represent two mixtures that did not separate when the temperature was somewhat raised (to near 650°); showing that with mixtures of bismuth, zinc, and cadmium, as with those of lead, zinc, and cadmium, a considerable depression of the upper part of the critical curve is brought about by a comparatively slight elevation of temperature.

	A.	B.	C.	D.	E.
Bismuth	23·34	20·29	18·71	21·50	21·05
Cadmium	54·49	57·38	53·89	52·20	51·00
Zinc	22·17	22·33	27·40	26·30	27·95
	100·00	100·00	100·00	100·00	100·00

On contrasting together the two critical curves thus obtained with cadmium as solvent it is noticeable that in this case also the same rule is observed as was found to hold in all the previously-described cases, viz., that, *cæteris paribus, the substitution of bismuth for zinc* as the heavier of the two immiscible metals *depresses the curve.* As regards the direction of slope of the tie lines, however, it is remarkable that with cadmium as solvent the ties slope uniformly to the *left* with *lead*, and to the *right* with *bismuth*, as the heavier immiscible metal. With silver as solvent all ties slope to the *left* whether lead or bismuth be the heavier immiscible metal, and whether zinc or aluminium be the lighter one; whilst with tin as solvent the opposite

is the case, the ties here all sloping to the *right*, with the exception of the lower ties in the case of the mixtures lead-tin-zinc and lead-tin-aluminium; this exception being presumably due, as previously described, to the influence of the endency towards the formation of a definite compound of lead and tin, Pb_3Sn.

Alloys containing Zinc with Lead (or Bismuth) and Antimony as Solvent.

These experiments were made in the same way as before, the only difference being that the temperatures at which the mixtures were kept in tranquil fusion lay between 600° and 700°, averaging near to 650°.

The resulting alloys were analysed by dissolving in diluted aqua regia, precipitating by sulphuretted hydrogen (after copious further dilution), separating antimony from lead (or bismuth) sulphide by repeated treatment with ammonium sulphide, and finally collecting on a weighed filter the mixed sulphur and antimony sulphide thrown down on acidulating the filtrate, and heating a known fraction of the dried precipitate in a current of carbon dioxide so as to expel sulphur and leave antimony sulphide. In the case of lead alloys the lead sulphide left undissolved was dissolved in nitric acid, evaporated with sulphuric acid, and the lead sulphate finally obtained weighed as such; when the acid filtrate from this contained small quantities of zinc (as occasionally happened, owing to zinc sulphide being carried down along with lead and antimony sulphides) this liquid was added to the first zinc-containing filtrate, and the two jointly precipitated with ammonia and ammonium sulphide, the zinc being ultimately transformed into carbonate, and weighed as ZnO as usual, correction being made for traces of Fe_2O_3 when present. In the case of bismuth alloys, the bismuth sulphide left undissolved by ammonium sulphide was dissolved in nitric acid, and precipitated boiling by ammonium carbonate. When zinc was present (carried down as sulphide, as before) this was chiefly found in the ammonium carbonate filtrate; sometimes, however, traces were carried down with this basic bismuth carbonate; these were separated by dissolving the weighed impure Bi_2O_3 in hydrochloric acid, evaporating to dryness, diluting the residue largely with water, and filtering off from the precipitated bismuth oxychloride.

In all cases the analyses were calculated taking the sums of the weights of the three metals found as 100.

Mixtures of Lead, Zinc, and Antimony.

The following average values were deduced from fourteen compound ingots (twenty-eight alloys), the proportion between lead and zinc throughout being near to equality:—

No. of tie-line	Heavier alloy.			Lighter alloy.			Excess of antimony percentage in lighter alloy over that in heavier.
	Antimony.	Lead.	Zinc.	Antimony.	Lead.	Zinc.	
0	0	98·76	1·24	0	1·14	98·86	0
1	2·69	93·87	3·44	4·60	2·27	93·13	+ 1·91
2	7·11	87·02	5·87	11·41	7·53	81·06	+ 4·30
3	8·37	85·22	6·41	16·75	13·52	69·73	+ 8·38
4	9·77	80·81	9·42	22·33	17·46	60·21	+12·56
5	15·45	67·17	17·38	26·68	24·34	48·98	+11·23
6	21·00	53·65	25·35	27·11	29·55	43·34	+ 6·11
7	22·80	48·48	28·72	28·15	30·36	41·49	+ 5·35

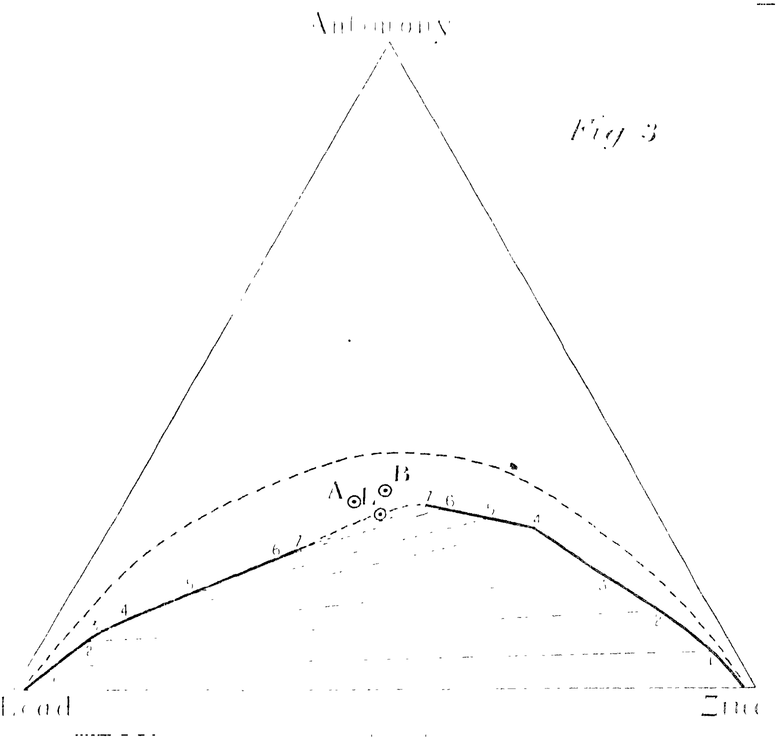


Fig. 3 represents these values plotted on the triangular system, the exterior dotted line indicating the corresponding curve obtained with tin as solvent instead of antimony (Part V). The points marked A and B represent two alloys that did not separate, containing

	A.	B.
Antimony	28·5	30·8
Lead	41·5	32·2
Zinc	30·0	37·0
	100·0	100·0

By means of Stokes' first method the values for the limiting point are deduced as being $A-B = 4$, $C+C' = 53$; whilst by means of the 2nd method the following values are found, $A+A' = 75$, $B+B' = 72$; leading to the final result

	1st Method.	2nd Method.	Mean.
Lead........	38·75	37·5	38·1
Zinc........	34·75	36·0	35·4
Antimony ..	26·50	26·5	26·5
	100·00	100·0	100·0

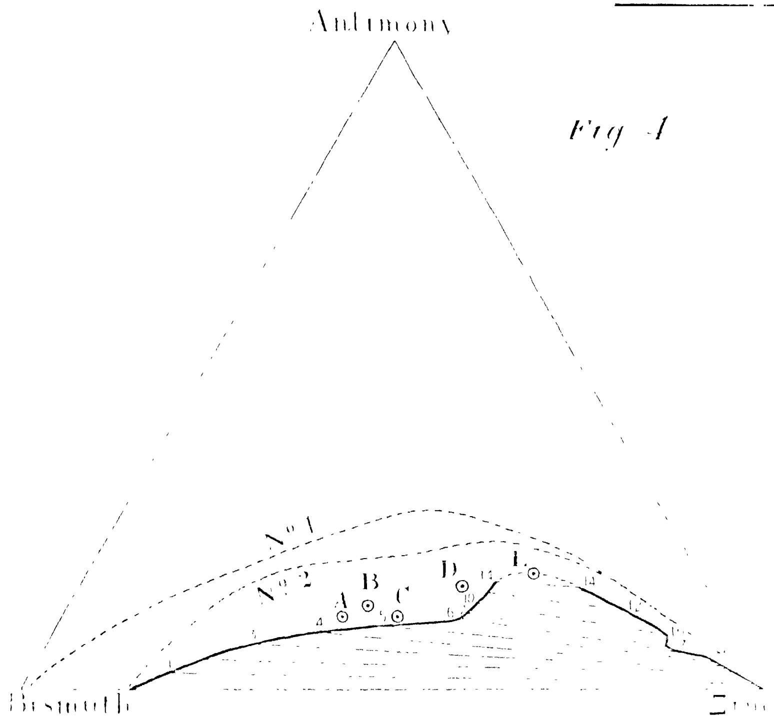
This indicates a ratio between lead and zinc near to that represented by the formula $PbZn_3$; the corresponding ratios found with tin, silver, and cadmium as solvent metals being respectively near to $PbZn_6$, Pb_2Zn, and Pb_4Zn.

Mixtures of Bismuth, Zinc, and Antimony.

The following average values were deduced from the examination of twenty-eight compound ingots (fifty-six alloys); in the earliest cases the ratio of zinc to bismuth was that of equality; later it was raised successively to 2 : 1 and 3 : 1, and finally to 7 : 2:—

No. of tie line.	Heavier alloy.			Lighter alloy.			Excess of antimony percentage in lighter alloy over that in heavier.
	Antimony.	Bismuth.	Zinc.	Antimony.	Bismuth.	Zinc.	
0	0	85·72	14·28	0	2·32	97·68	0
1	1·55	81·23	17·22	0·43	3·23	96·34	−1·12
2	4·10	70·02	25·88	2·15	4·75	93·10	−1·95
3	6·33	60·67	33·00	3·09	5·07	91·84	−3·24
4	8·63	53·00	38·37	3·67	6·02	90·31	−4·96
5	10·03	44·05	45·92	4·32	8·02	87·66	−5·71
6	10·90	37·09	52·01	5·63	10·11	84·26	−5·27
7	11·74	35·05	53·21	5·54	11·26	83·20	−6·20
8	12·84	33·03	54·13	5·69	10·47	83·84	−7·15
9	13·22	33·20	53·58	6·47	9·76	83·77	−6·75
10	14·12	32·66	53·22	7·02	8·65	84·33	−7·10
11	15·87	30·67	53·46	9·25	10·37	80·38	−6·62
12	16·04	28·61	55·35	12·81	15·19	72·00	−3·23
13	16·75	27·66	55·59	14·43	17·56	68·01	−2·32
14	17·00	26·58	56·42	15·53	18·28	66·19	−1·47

Fig. 4 represents these values, the outer dotted line, No. 1, indicating the corresponding curve for lead-zinc-antimony alloys, and that marked No. 2 the similar curve for bismuth-zinc-tin alloys (Part V), all referring to the same average temperature of 650°, or thereabouts.



The points marked A, B, C, D indicate four mixtures that did not separate; viz.:—

	A.	B.	C.	D.
Antimony	11·71	14·6	11·6	15·63
Bismuth	50·17	42·7	44·2	30·89
Zinc	38·12	42·7	44·2	53·48
	100·00	100·0	100·0	100·00

By means of Stokes' second method, the limiting values are deduced, $A+A' = 45\cdot5$, $B+B' = 119\cdot5$; whence the composition at the limiting point is—

Antimony............	17·50
Bismuth.............	22·75
Zinc................	59·75
	100·00

Here the bismuth and zinc are in proportions near to those indicated by the formula $BiZn_8$; with tin, silver, and cadmium as solvent metals the corresponding proportions were respectively near to those indicated by $BiZn_{10}$, $BiZn_2$, and $BiZn_5$.

It is noticeable that the limiting point thus deduced is the highest point of the entire curve; whereas, in every one of the other eleven curves so far investigated, the limiting point lies on one side or the other of the highest point, and more or less lower down.

Two remarkable irregularities are visible in the contour of the above-deduced critical curve for antimony-bismuth-zinc alloys; on the right-hand side a peculiar notch or depression is noticeable, strongly suggesting a phenomenon similar in character to that previously observed in the case of silver-zinc-lead and silver-zinc-bismuth alloys; *i.e.*, the formation of a definite compound of solvent with one of the two immiscible metals more soluble in the other of these two metals than any other neighbouring mixture of the two in other proportions. On the left-hand side an analogous, but far wider, depression is observable. These depressions reach their greatest depths at the 7th tie line, where at each of the two conjugate points the ratio between the bismuth and antimony present is not far from that corresponding with the formula Bi_3Sb_2.

	Left hand (heavier alloy).			Calculated for Sb_2Bi_3.	Right hand (lighter alloy).		
Antimony..	11·74	=	25·1	27·8	5·54	=	32·9
Bismuth...	35·05	=	74·9	72·2	11·26	=	67·1
	46·79		100·0	100·0	16·80		1·000

Experiments now in progress indicate that an analogous depression exists in the critical curve obtained with antimony-bismuth-aluminium alloys, also most strongly marked at a point where the bismuth and antimony are in proportions not far from that corresponding with Bi_3Sb_2.

On comparing together the two curves deduced with antimony as solvent it is noticeable that they show the same general characters as the corresponding pair of curves similarly obtained with cadmium as solvent; *i.e.*, whilst the curve with bismuth as heavier immiscible metal lies *inside* that with lead (as in all other similar cases as yet examined), the direction of slope of tie lines is opposite in the two

cases; viz., uniformly to the *right* with *bismuth* and to the *left* with *lead*. It is remarkable, however, that this relationship does not hold when aluminium is substituted for zinc, the experiments now in progress indicating that the ties then *always slope to the left*, whether lead or bismuth be the heavier immiscible metal.

On comparing together the three sets of critical curves deduced for temperatures not far apart (600—650°) with tin, cadmium, and antimony respectively as solvent metal, it is noticeable that whether bismuth or lead be the heavier immiscible metal the curve with cadmium as solvent lies *outside*, and that with antimony *inside*, the curve deduced with tin as solvent. The curves obtained with silver as solvent cannot properly be directly compared with these on account of the higher temperatures (800—870°) employed; but, judging from the marked effect of a rise of temperature in depressing the critical curves obtained with cadmium as solvent, it seems probable that for the same temperature the curve with silver as solvent would be found to lie *outside of that with cadmium as solvent*, the two immiscible metals being the same. At any rate, in all cases the curve with silver as solvent lies far outside that similarly obtained with tin as solvent.

In the case of alloys containing aluminium as lighter immiscible metal, it has been shown (Part VI) that with silver as solvent the critical curve also lies *outside* that obtained with *tin* as solvent, whether bismuth or lead be the heavier immiscible metal. The experiments now in progress seem to indicate that the corresponding curves with antimony as solvent lie again *inside* the curves deduced with tin as solvent. As already stated, corresponding curves with cadmium as solvent cannot be obtained, as the immiscibility of aluminium and cadmium causes the resulting ternary alloys to belong to an entirely different class, the critical curves pertaining to which cannot be directly compared with those belonging to ternary mixtures analogous to the twelve so far investigated.

Much of the analytical work requisite for the above experiments was carried out by Mr. Sydney Joyce, to whom the author's acknowledgments are due for the assistance rendered.

Presents, February 2, 1893.

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February 9, 1893.

Sir JOHN EVANS, K.C.B., D.C.L., LL.D., Vice-President and Treasurer, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Preliminary Account of the Arrangement of the Sympathetic Nervous System, based chiefly on Observations upon Pilo-motor Nerves." By J. N. LANGLEY, F.R.S., Fellow of Trinity College, Cambridge. Received January 21, 1893.

[PLATE 14.]

I propose to give here a general statement of some of the conclusions which I have come to with regard to the arrangement of the sympathetic nervous system, reserving detailed treatment for a later time. As a starting point I take the distribution of the pilo-motor nerves in the cat.*

In the cat, the spinal nerves which contain pilo-motor fibres in their nerve-roots, are usually the 4th thoracic to the 3rd lumbar inclusive. Such fibres are, in rare cases, present in the 3rd thoracic nerve, and occasionally in the 4th lumbar nerve.

* A certain number of facts with regard to this, I have given in an earlier paper written in conjunction with Dr. Sherrington. Dr. Sherrington dealt with the pilo-motor nerves in the monkey (*cf.* 'Journ. of Physiol.,' vol. 12, p. 278, 1891).

The spinal pilo-motor fibres run into the sympathetic trunk, there they become connected with nerve-cells; on leaving the sympathetic chain, they run to their peripheral endings in cranial or spinal nerves. The fibres to the body accompany those dorsal cutaneous branches of the spinal nerves which supply the skin over and close to the vertebræ.

Broadly speaking, the pilo-motor fibres run from the sympathetic chain to the cranial and spinal nerves in the grey rami, but a few fibres may run out in the white rami; and in some of the upper thoracic rami, as is well known, no anatomically separable white rami occur.

Broadly speaking, the fibres issuing from any one ganglion are connected with nerve-cells in that ganglion, and with no other sympathetic nerve-cell. In some cases a certain number of such fibres are connected with nerve-cells, not in the ganglion from which they issue, but in the ganglion immediately above or below it. In the following statement these fibres, and those which may take the course of the white rami, are, for the sake of simplicity, left out of account.

The fibres, before and after they have joined nerve-cells, I shall call respectively pre-ganglionic or pre-cellular, and post-ganglionic or post-cellular.

Each ganglion, by its post-ganglionic fibres, supplies, in any one individual, a definite portion of skin. This portion varies somewhat in different individuals. The more important variations only will be mentioned here.

The areas supplied by the ganglia from above downwards, starting with the superior cervical ganglion, are, apart from a variable amount of overlapping, successive areas.

An overlapping of the areas occurs when one nerve receives post-ganglionic fibres by more than one ramus; thus the 3rd cervical nerve, and so the skin of the upper part of the neck, may receive pilo-motor fibres from the superior cervical ganglion by one grey ramus, and from the ganglion stellatum by another grey ramus; not uncommonly the 7th lumbar nerve, and so the skin over the upper coccygeal vertebræ, receives pilo-motor fibres by two separate grey rami, one from the 7th lumbar ganglion, and the other from the 1st sacral ganglion. Similarly, also, a lower thoracic, or upper lumbar, nerve may receive pilo-motor fibres by the grey ramus of its own ganglion, and others by the white ramus which it gives off to the ganglion below.

A second cause of overlapping is a spreading out of the pilo-motor fibres in the skin itself. When two successive grey rami, or two successive dorsal cutaneous branches, are stimulated one after the other, the area of skin affected by both of them may be not more than $\frac{1}{2}$ to 1 mm.; but it may be about 5 mm., and in such case the

"exclusive" area of a ramus may be about a half only of the total area supplied by it.

A third cause of over-lapping may be the formation of a plexus in the dorsal cutaneous branches of the spinal nerves; this is, perhaps, the cause of some of the cases of over-lapping of areas which I have observed in the sacral and coccygeal regions.

Most of the details with regard to the over-lapping of the areas we may leave on one side, and, treating the areas as successive, proceed to consider their position.

The cranial rami of the superior cervical ganglion supply the skin of the dorsal part of the head, except a posterior portion, beginning about $1\frac{1}{2}$ cm. behind the anterior level of the ears; this unaffected region we may call the occipital region.

The cervical rami of the superior cervical ganglion supply the skin of the occipital region of the head by fibres running in the great occipital (2nd cervical) nerve, and the skin over the first three or four cervical vertebræ by fibres running in the 3rd cervical nerve.

The ganglion stellatum, by its cervical rami, supplies the skin from the 3rd and 4th cervical vertebræ to some point between the spine of the 2nd and 3rd thoracic vertebræ. Often its area extends upwards to join the occipital region.

The areas supplied by the post-ganglionic pilo-motor fibres of the 3rd 4th, 5th, and 6th cervical nerves vary in relative size in different individuals; roughly, we may take the 3rd nerve as supplying the skin over the first three and a half vertebræ, and the others as supplying successive strips of about two vertebræ each.

We come now to the fore leg region, in which one, two, or three spinal nerves send no cutaneous branches to the mid-line of the back. These are the 7th and 8th cervical, and the 1st thoracic, nerves.

Sometimes the 7th, sometimes the 1st, thoracic has such a cutaneous branch; when it is present it contains pilo-motor nerves. I have not, in any case, observed any such cutaneous branch from the 8th cervical nerve. Corresponding to the presence or absence of these cutaneous branches—so far as my experiments go—is the presence or absence of pilo-motor fibres in the rami which pass from the ganglion stellatum to the respective nerves. Thus, if the 1st thoracic nerve sends a branch to the skin over the vertebræ, stimulation of its ramus, as of the branch itself, causes a movement of hairs.

In the two experiments in which there was a mid-line cutaneous branch from the 7th cervical nerve, it supplied the lower part of the area usually supplied by the 6th nerve, the area of this and of the 5th and 4th nerves lying a little more anteriorly than usual.

The ganglion stellatum also sends pilo-motor fibres to the first four thoracic nerves. From the 5th thoracic nerve downwards (and sometimes from the 4th) there is a ganglion and ramus for each nerve.

The distribution of all these rami down to the 4th lumbar we may consider together.

The area of the 2nd thoracic ramus (or of the 1st, as mentioned above) follows on the area of the lowest effective cervical ramus. The 4th lumbar ramus supplies either the skin over the 7th lumbar vertebra and a small piece of sacrum, or the skin over the sacrum. Between the limits just given for the 2nd thoracic and the 4th lumbar the areas follow on each other, the length of each area being about that of a vertebra. There are variations in the relative length of the areas in different animals, but, generally speaking, we may say that in the thoracic region the areas are a little larger than the neighbouring vertebræ, and in the upper lumbar region a little shorter.

Below the 4th lumbar nerve we reach the hind leg region, which is like that of the fore leg already mentioned, in so far as one, two, or three nerves have no dorsal cutaneous branches to the mid-line, and the corresponding rami have no pilo-motor fibres. These nerves are the 5th, 6th, and 7th lumbar. On the rami of these nerves I have made many more experiments than on the rami of the ganglion stellatum, and, no doubt in consequence, I have found greater variation. Thus, as I have said, I have not so far observed any case in which the ramus to the 8th cervical nerve contained pilo-motor fibres. In the lumbar region the 6th nerve is that which most frequently has no pilo-motor fibres, but in two cases it apparently did contain some; it is worth mention that in these cases the lumbo-sacral plexus was an extreme form of my Class I* (posterior type of arrangement, part of Sherrington's† post-fixed plexus), the 7th nerve sending a filament to the obturator. The 5th lumbar ramus has pilo-motor fibres more commonly than the 7th.

About the end of the sacrum appears to be the dividing line between the areas of the rami which come from above and those which come from below the ineffective ramus or rami. Thus the skin over the lower part of the sacrum may be supplied by the 4th, 5th, or, perhaps, the 6th lumbar ramus, the skin over the upper coccygeal vertebræ by the 7th lumbar or 1st sacral. In each case there are one or more nerves, lying between the nerve which supplies the lower part of the sacral region and that which supplies the upper coccygeal region, which contain no pilo-motor fibres. This dividing line in the experiments in which I have specially noted the point has only varied from a position midway between the spine of the 2nd and 3rd sacral vertebræ to a position a little below the spine of the 3rd sacral vertebra. As I have indicated earlier, the corresponding line in the upper thoracic region also appears to undergo very slight variations.‡ All the

* 'Journ. of Physiol.,' vol. 12, p. 350, 1891.

† *Ibid.*, vol. 13, p. 635, 1892.

‡ *Cf.* also p. 555, figs. 1 and 2.

other dividing lines between the areas vary not inconsiderably in position.

The 2nd sacral ramus, as a rule, supplies the hairs of the tail just above the level of the anus and over it; the 3rd sacral ramus supplies the hairs for about an inch and a half below the level of the anus. The coccygeal ganglion gives off rami to the several coccygeal nerves, and these supply different lengths of the tail. This ganglion supplies by its rami the whole, or nearly the whole, of the tail.

The distribution of the pilo-motor post-ganglionic fibres of each spinal nerve, *i.e.*, of the fibres which run to each spinal nerve from the sympathetic system, can also be determined by stimulating the nerve inside the spinal canal. This method depends upon the spreading of the current down the nerve to its dorsal branch, in which the pilo-motor fibres run. In most cases rather strong shocks are required; in others, as in the sacral region, moderate shocks are sufficient. A similar spreading down of the current to the ventral branches of the nerves may perhaps be the explanation of the assertion that the lower lumbar nerves contain direct spinal secretory and vaso-motor fibres for the foot. When a spinal nerve sends pilo-motor fibres (pre-ganglionic fibres) to the sympathetic, as well as receiving some from it (post-ganglionic fibres), the two can readily be distinguished by injecting nicotine. This cuts out the effect of the pre-ganglionic, but not that of the post-ganglionic, fibres.

So far, I have been chiefly concerned with the description of the portion of the dorsal skin innervated by each ganglion through its grey ramus. The next question to consider is the connexion of each ganglion with the spinal cord. Like the other questions dealt with here, this must be treated somewhat broadly, since there are more or less pronounced individual variations.

In the following table I give what appear to be the ordinary connexions* in an animal in which the 4th lumbar nerve and the 6th lumbar sympathetic grey ramus contain no pilo-motor fibres. I omit the connexion of the 4th and 5th spinal nerves with the sympathetic ganglia. I insert the connexions of some spinal nerves which do not contain pilo-motor fibres. In the left-hand vertical column, the numbers refer to the spinal nerves arranged in order, beginning with the 1st thoracic. The numbers placed in a horizontal line with the number of each spinal nerve represent the sympathetic ganglia in which its fibres make connexion with nerve-cells to issue in the corresponding grey rami. S.c.g. and g.st. are used respectively for the superior cervical ganglion and the ganglion stellatum. When either of these, or the number of a ganglion, is enclosed in brackets,

* The connexion of the nerves with the uppermost and lowermost of their series of ganglia is sometimes slight only; this is especially the case as regards the connexion of the lumbar nerves with the uppermost of their series of ganglia.

Usual Connexions of the Visceral Fibres of the Spinal Nerves with the Ganglia of the Sympathetic Chain, the 4th and 5th Lumbar Nerves being omitted. (*Cf.* Plate 14.)

I	(s.c.g.)																					
II	(s.c.g.)																					
III	(s.c.g.)																					
IV	s.c.g.	(g.st.)																				
V	s.c.g.	g.st.																				
VI	s.c.g.	g.st.																				
VII	s.c.g.	g.st.	5	6																		
VIII		g.st.	5	6	7	8	9															
IX		(g.st.)	5	6	7	8	9	10	11													
X							9	10	11	12	13	1										
XI									11	12	13	1	2	3								
XII											13	1	2	3	4	5	(6)					
XIII												1	2	3	4	5	(6)	7				
I													2	3	4	5	(6)	7	1	2		
II														3	4	5	(6)	7	1	2	3	1 (part)
III																5	(6)	7	1	2	3	1

it indicates that the connexion of the spinal nerve with the ganglion has been determined, not by pilo-motor, but by some other, fibres.

The facts given in the table, together with those given earlier regarding the distribution of the rami of the ganglia, show the areas of the skin which are supplied by the pilo-motor fibres issuing from the cord in the roots of each spinal nerve given in the table.

Can we from these data deduce any conclusions with regard to the distribution of the other sympathetic fibres of the spinal nerves?

I have said that the pilo-motor fibres to the skin over the vertebræ run to the periphery in the dorsal cutaneous branches of the spinal nerves. It is easily shown that the area of the skin supplied with pilo-motor fibres by the dorsal cutaneous branch of any given spinal nerve is also supplied by it with sensory fibres. And I think there is good reason for believing that the fibres of the grey ramus of a nerve, *i.e.*, the post-ganglionic sympathetic fibres of a spinal nerve, have, in the main, the same distribution in the skin as the sensory fibres of the nerve.

The chief physiological observations on the distribution of sensory fibres in the skin are those of Türck,* made on the dog, and of Sherrington,† made on the monkey. These observations give the sensory areas of both dorsal and ventral cutaneous branches. To compare with these, we have the pilo-motor nerves of a portion of the dorsal cutaneous branches, and the secretory nerves of certain ventral cutaneous branches running to the hind foot.‡ There would be no advantage in attempting here to discuss in detail the degree of coincidence in the areas of the sensory and of the visceral fibres, and chiefly because, in many cases at any rate, a given spinal nerve in any one of the animals has not an exactly corresponding spinal nerve in the other two animals. But, in my opinion, the correspondence in the areas of the sympathetic and the sensory fibres, so far as we know them, is close enough to justify the conclusion that the sensory and the post-ganglionic sympathetic fibres in any spinal nerve have, in the main, the same distribution. The chief point which may be urged against this view is that, according to Sherrington, the areas of the sensory fibres of the dorsal and upper lumbar spinal nerves largely overlap.§ An overlapping of pilo-motor fibres as great as that found by him for sensory fibres has not occurred in my experiments. On the other hand, it is to be remembered, that in my experiments—

* Türck, 'Sitzungsb. d. Wiener Akad.,' vol. 21, 1856; and 'Denkschrift. d. Wiener Akad.,' vol. 29, 1869.

† Sherrington, 'Roy. Soc. Proc.,' vol. 52, p. 333, 1893.

‡ 'Journ. of Physiol.,' vol. 12, p. 347, 1891.

§ Türck did not find overlapping, or only slight overlapping, in the nerves of the neck and trunk.

whenever the matter was tested—the pilo-motor area of a cutaneous branch was found to be supplied with sensory fibres from the same branch; that there is a certain amount of overlapping in the pilo-motor areas of successive cutaneous branches; and that the sympathetic fibres of any given kind may have a rather less or rather greater extension than the sensory fibres with which they run.

Of the distribution of sympathetic fibres to muscles we have no information, but, if the view I have given above is true, it establishes a probability that such of these fibres as may run in a given grey ramus to a nerve would have approximately the same distribution as the muscular branches of the nerve.

We have seen that at the origin of the nerves for the arm and for the leg, one, two, or three grey rami contain no pilo-motor fibres. Let us consider a simple case, say, when the 6th lumbar grey ramus has no pilo-motor fibres. Five spinal nerves, the 12th, 13th thoracic, the 1st, 2nd, 3rd lumbar, give off nerve-fibres, which leave the sympathetic by each of the grey rami of six or seven ganglia in the neighbourhood of, and including, the 6th lumbar ganglion.* Of this series of rami, the 6th lumbar is about the last supplied by the 12th thoracic, and about the first supplied by the 3rd lumbar nerve (*cf.* table above). Further, each of these spinal nerves has abundant pilo-motor fibres, and sends them to every ramus except the 6th lumbar. Why is it that the pilo-motor fibres of each nerve skip this particular ramus, skipping it wherever it comes in the series of rami supplied by the nerve? And, further, why do variations occur in different animals, so that any one or all of the grey rami of the 5th, 6th, or 7th lumbar ganglion may have no pilo-motor fibres?

The proximate explanation I take to be that the sympathetic ganglia develop in connexion with the spinal nerve-roots; † that, as is now generally believed, the number of consecutive nerve-roots passing out in a particular spinal nerve varies; that the sympathetic fibres issuing from the sympathetic ganglion to run to the nerve follow in the main the course of the nerve; and that probably the connexion of the ganglion with the cord by the white rami is a subsequent event. The following diagram represents the chief points of this explanation.

A detailed account of the facts summarised above I hope to be able to give in no long time, and then, also, I hope to deal with one or two problems of the sympathetic system which I have omitted here.

Cf. Langley, 'Journ. of Physiol.,' vol. 12, p. 347, 1891.

† According to His and others, a common mass of cells gives rise to both sympathetic and spinal ganglia.

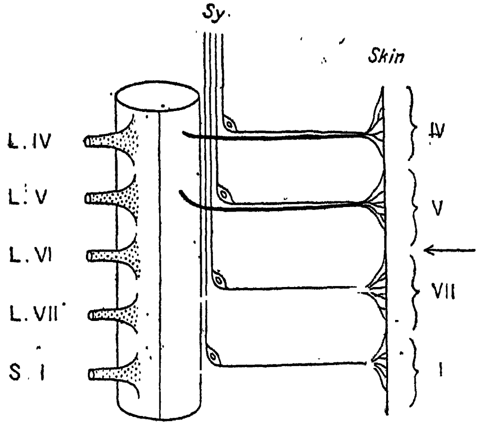


FIG. 1 is a diagram of the course of the pilo-motor and sensory fibres to the skin near the mid-dorsal line, when the 6th grey ramus contains no pilo-motor fibres. The thick lines represent the sensory spinal fibres; the thin lines accompanying them the sympathetic fibres. The arrow represents a line of skin at about the end of the sacrum.

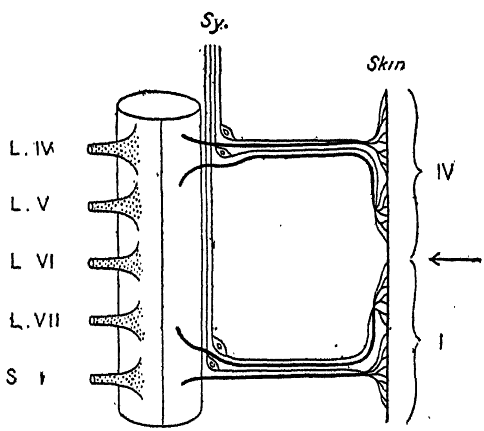


FIG. 2 is a diagram similar to that of fig. 1, but for the case when the 5th, 6th, and 7th grey rami have no pilo-motor nerves. The shifting which commonly occurs of some fibres of the 4th lumbar into the 3rd, and of some fibres of the 1st sacral into the 2nd, is here left out of account.

DESCRIPTION OF PLATE 14.

Diagram to show the connexions of—

(*a*.) The spinal nerves with the ganglia of the sympathetic chain.

(*b*.) The grey rami of the ganglia with the skin.

The diagram also shows the distribution of the sensory fibres of the spinal nerves with the skin of the mid-dorsal line; leaving out of account any overlapping of the areas that may occur.

The meaning of the several parts of the diagram is, for the most part, explained by the lettering.

In addition it must be noted that—

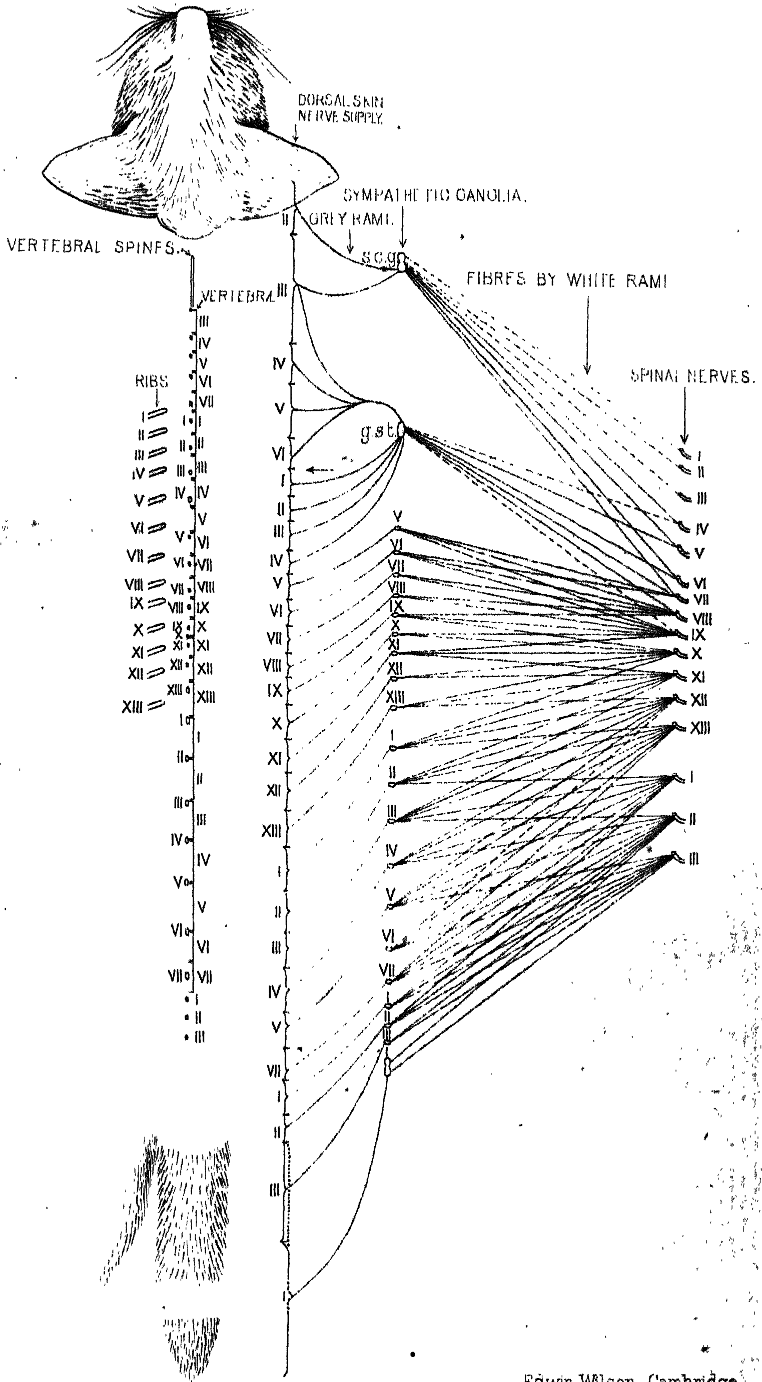
1. The broken lines indicate that the connexion shown by the line has been determined by experiments on some other than pilo-motor fibres, *e.g.*, by pupillo-dilator or by sweat-nerve fibres.
2. The connexion of the several spinal nerves with the sympathetic ganglia is not in all cases as it is represented in the diagram. There are individual variations.
3. Any one spinal nerve is connected with certain of its ganglia by a considerable number of fibres, and with others by very few. It is connected by few fibres with either the uppermost or the lowermost, or with both uppermost and lowermost, of its series of ganglia. Thus the 7th thoracic nerve sends comparatively few fibres to the superior cervical ganglion, and the 3rd lumbar nerve sends comparatively few fibres to the 5th lumbar sympathetic ganglion.
4. The distribution of the several grey rami to the skin also varies; the extent of this variation has been described in the text. In the scheme given here, the case is taken when the 7th and 8th thoracic and the 6th lumbar nerves do not run to the mid-line of the back.
5. The dotted line stretching from the upper end of the skin area of the grey ramus of the 1st coccygeal ganglion indicates that the area of the 3rd sacral grey ramus is usually supplied by fibres from the coccygeal ganglion. It will be noticed that only the base and tip of the tail are represented.

II. "Note on the Knee-jerk and the Correlation of Action of Antagonistic Muscles." By C. S. Sherrington, M.A., M.D. Communicated by Professor M. Foster, Sec. R.S. Received February 1, 1893.

(From the Physiological Laboratory, St. Thomas's Hospital.)

The muscular reaction known as the knee-jerk is notoriously affected by conditions obtaining in what is often described as a reflex arc, consisting of afferent and efferent paths, and a centre situate in the lumbar portion of the spinal cord. I recently* described experiments determining more particularly than hitherto the locality of the muscular and nervous mechanism on which the jerk depends. I showed that the muscular portion of this mechanism consists mainly

* 'Journal of Physiology,' vol. 13, p. 666.



Edwin Wilson. Cambridge.

of the *vastus internus* and part of the *crureus* divisions of the great quadriceps extensor muscle of the thigh. The *spinal centre* was found located in the 5th and 4th lumbar segments of the cord of the *Rhesus* Monkey (4th and 3rd lumbar of Man). The *efferent* path was found in the anterior roots of the 5th and 4th lumbar nerves, and was traceable along the anterior crural nerve into those of the muscular branches of that trunk which supply the above-mentioned portions of the quadriceps extensor max. The efferent side of the path corresponds accurately with the course of the motor nerve fibres to the muscles in question, and there is little reason to doubt that it consists of nothing more or less than of those motor fibres themselves. The *afferent* path was found to lie in the posterior root of the 5th lumbar of *Rhesus* (4th of Man, 6th of Cat), and was not usually demonstrable at all in the posterior root of the 4th lumbar, but a small portion of it may, perhaps, lie within that root.

The posterior root in which exists the afferent path on which the jerk is dependent receives afferent fibres from the obturator and anterior crural nerves, and from the external and internal popliteal nerves, and sometimes from the division of the great sciatic which may be called the hamstring nerve, because distributed to the hamstring muscles. Of the fibres entering the root from these various sources, those on which the "jerk" depends are not from any except the anterior crural nerve. Further, in the anterior crural nerve, they are those fibres of the nerve which issue from the vastus internus and crureus muscles. Thus the afferent fibres on which the jerk depends seem to arise within muscles, and from exactly those muscles, to which belong the efferent fibres with which the "jerk" is concerned.

The rapid abolition of the jerk produced by severing the posterior root of the 5th lumbar of *Rhesus* may conceivably be due less to mere interruption of an afferent path than to excitation of an afferent path by the "current of injury" set up in the injured fibres ("demarcation current") of it. This doubt has frequently been strengthened in my mind by the fact that section of one half the root often suffices to abolish the "jerk," although the remaining half can, when tested, still be shown able to conduct centripetal impulses from the skin; and further, by the fact that it appears immaterial whether the anterior or the posterior part of the posterior root be selected for the section. The "jerk" I have seen then abolished in a manner not obviously different from that in which section of the whole root abolishes it. Against such an explanation is, however, the permanence of the effect upon the "jerk" produced by section of the whole root, for the effect continues at least for many days. Regarding the permanence of the effect of section of half the root I have no observation.

I have repeated the observation substituting for severance other modes of destruction of the conductivity of the root. The root is a fairly long one, longer in the Monkey than in the Cat, and it is not difficult to apply reagents to it. I find the jerk immediately abolished by cooling the root to near the freezing point. To do this I pass under the posterior root, well lifted from the anterior, one end of a copper strip, the other end of which lies in an ice and salt mixture. The application of CO_2 vapour to the root has a similar effect, and on removing the vapour the "jerk" returns. The vapour I have applied through a thin-walled india-rubber tube, made to enclose the root. Cocain I have also applied, and found it abolish the jerk in about 70 secs., when used as a 1 per cent. solution in 0·6 per cent. sodium chloride solution. I place under the root, before applying the cocain, a thin strip of india-rubber sheeting, and apply the solution with a fine camel's-hair brush by painting on the filaments of the root.

There seems, therefore, no doubt that abolition of the jerk can be produced by lowering the conductivity of the fibres of this posterior root. Whatever the nature of these afferent fibres which thus come up from part of the quadriceps extensor of the thigh, and keep the "knee-jerk" going, facts show that they are less hardy under experimental interference than are those from the skin which carry centripetal impulses subserving tactile sensation. A very little interference with this posterior root abolishes the knee-jerk; a very great deal will often not obviously impair cutaneous reflexes elicited through it. To lift the posterior root by a thread passed under it will often suffice to interrupt the afferent fibres for "the jerk," but at the same time leaves the afferents of tactile sense not obviously impaired. Probably the former fibres are much the smaller and more delicate.

The irritation of this root, when cut, by its own demarcation current does not cause inhibition of the jerk. I have tried on three occasions to recover the "jerk," after its disappearance on section of this root, by electrical excitation of the central end of the divided root. The excitation, when too feeble to elicit any reflex contraction of the muscles, did not obviously influence the briskness of the jerk in either direction. The excitation very readily, however, causes contraction of the *hamstring* muscles, which so alters the position of the knee that the condition of the "jerk" can no longer be satisfactorily compared with what it was before.

Excitation of the central end of the divided hamstring nerve does at once abolish, or greatly reduce, the briskness of the "jerk." I have elsewhere* described a curious fact concerning the "jerk," namely, that it can be rendered brisk by section of afferent, or of

* 'Journ. of Physiol.,' *loc. cit.*

efferent, spinal roots immediately below that one on which the jerk itself depends. I added, with regard to it, "Its explanation appears to lie in the abolition of the tone of the hamstring muscles by section of the afferent roots belonging to them." I wish now to support, and somewhat enlarge, the explanation then offered.

Severance of the great sciatic trunk produces, as Tschiriew* has pointed out in the Cat, an increased briskness of the "jerk." This I find to depend scarcely at all, if indeed at all, on section of the external or internal popliteal divisions of the trunk, either singly or together; but to depend upon the cutting that portion of the trunk which is destined for the hamstring muscles—the portion referred to in my previous paper as "the hamstring nerve." In *Macacus* this "hamstring" division of the sciatic sends afferent fibres into the spinal cord by the posterior roots of the 8th, 7th, and 6th subthoracic nerves. In Cat, the 8th and 7th posterior roots are those in question. On severance of these afferent roots, the "tonus" of the hamstring muscles is broken, and the "jerk" becomes more brisk; sometimes there is a short interval of depression immediately succeeding the operation. The motor fibres of the hamstring muscles leave the cord by the anterior roots, correspondent with the abovementioned posterior. Severance of these anterior roots causes immediate increase in the briskness of the jerk.

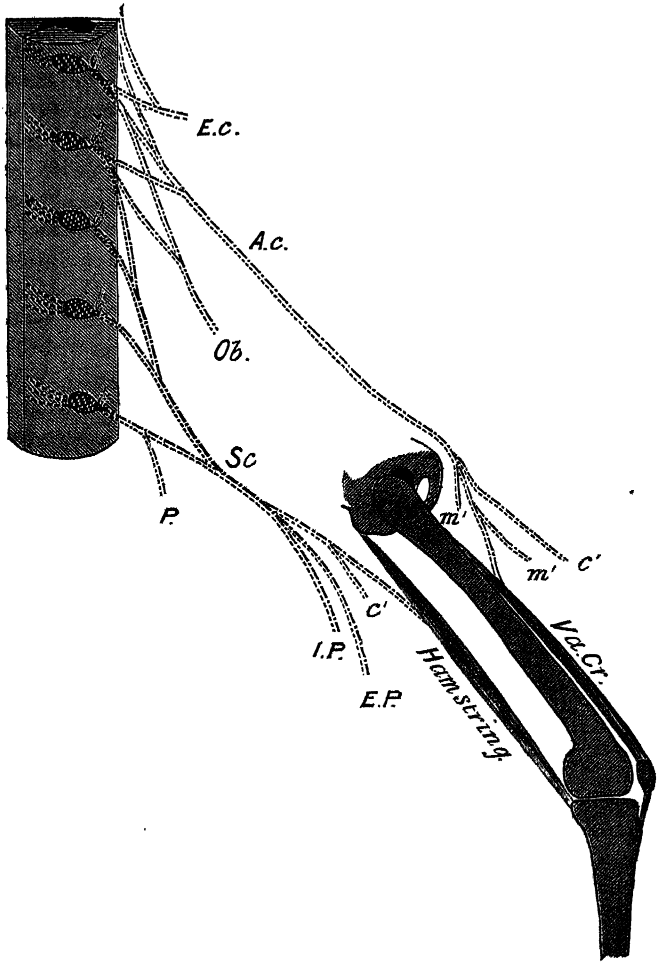
As to the manner in which the loss of tonus of the hamstring muscles gives rise to increase of the knee-jerk, two possibilities at once present themselves. One is purely mechanical; the other is of a physiological nature. The loss of tension accompanying the loss of "tone" will leave the leg more free to swing at the knee joint. It is for that reason that the posture of limb usually employed as the most favourable when the jerk is to be elicited is with the hamstrings relaxed and the leg at a right angle with the thigh. In this way the points of bony attachment of the hamstring muscles are approximated, and the knee can swing through a greater arc to that point at which it is cut short by the mechanical check of the flexor muscles passively tightened by the movement. The extensor movement during the "jerk" has, in this way, further scope before the hamstrings break it. As far as this explanation goes, the abovementioned increase of the jerk would occur even if the hamstring muscles were replaced by india-rubber cords, the effect produced by severance of the hamstring nerve being equivalent to any arrangement which rendered those india-rubber cords less tight.

I should have considered this simple explanation sufficient had it not been for certain additional facts. During experiments on the effects of stimulating the motor spinal roots of the lumbo-sacral nerves there is much risk of being deceived by escape of the excit-

* 'Arch. f. Psych.,' vol. 8.

ing current to other motor roots besides the one to which the electrodes are applied. When stimulating the motor root of the 7th lumbar, I frequently observed contraction of the extensor muscles of the knee as well as of the flexors, and imagining that the phenomenon must be due to escape of the exciting current to the 5th lumbar root, I was accustomed to reduce the strength of the exciting current until the contraction of the extensors of the knee no longer occurred. To avoid this supposed escape of current, it was necessary to reduce the strength of stimulus sometimes to very slightly indeed above minimal efficiency for the motor fibres to which the electrodes were applied. The use of such weak currents has serious disadvantages, and was extremely embarrassing for the experiment. It was not until I had discarded a number of experiments on the ground of escape of current, that three points concerning the contraction of the extensor muscles produced by stimulating the motor nerve to the flexors attracted my attention. (1.) If for the excitation of the motor root to the flexors a series of induced currents are employed, succeeding each other at a rate slow enough to produce not perfect tetanisation, but tremulent contraction of the muscles, the contraction obtained in the extensor muscles coincidently was, nevertheless, perfectly steady and tetanic, although not vigorous. (2.) If the flexor muscles are severed from connexion with the knee joint, so that their contraction cannot affect the joint, and if the "knee-jerk" be elicited before, during, and after stimulation of the motor root to the flexor muscles, *during the excitation*, when those flexor muscles were contracting, the knee-jerk, brisk previously and brisk later, disappeared, or almost disappeared. (3.) If the sensory spinal roots belonging to the hamstring nerve are severed, the stimulation of a motor root to the hamstring muscles is no longer accompanied by contraction of the extensor muscles of the knee, even when strong stimulation is employed.

One next observed the effect on the extensors of the knee of excitation of the central end of the nerve to the hamstring muscles after that nerve had been ligated and cut through. It was found that excitation with currents just perceptible at the tip of the tongue causes immediate *disappearance* or diminution of the "knee-jerk." "Exaltation" of the jerk follows the depression by the excitation. If the excitation be continuously maintained for a time, the jerk tends to return in spite of the continuance of the stimulation. By use of stronger currents the extensors are immediately thrown into a tonic contraction, lasting so long as the stimulus is continued. The same effects on the knee-jerk, and on the activity of the extensor muscles, are elicited by exciting the central ends of the divided posterior roots of the 7th or 8th subthoracic nerve.



E.c. External cutaneous nerve trunk.
A.c. Anterior crural nerve trunk, with *c*, cutaneous, and *m'*, muscular, branches.
Ob. Obturator nerve-trunk.
Sc. Sciatic nerve trunk, with *I.P.*, internal popliteal, *E.P.*, external popliteal, divisions, and the division going to the hamstring muscles, which gives a cutaneous branch *c'*.
Va.Cr. The vasti and the crureus muscles, the internal portion being especially referred to.

I then attempted to determine if mere tension of the hamstring muscles could give the same result as electrical excitation of the central ends of their nerves. It is, of course, essential that the production of increased tension in the muscles should not alter the

position of the knee joint or affect it mechanically. This precaution was observed by isolating the two inner hamstrings from their attachments, except at their origin, from the *tuber ischii*, and simultaneously cutting through all nerve branches to the outer hamstrings and to the adductor muscles. The nerves to the inner hamstrings were carefully preserved although the muscles were otherwise dissected out. It was found that by pulling on the inner hamstring muscles sufficiently to stretch them out of the doubled-up shape they assumed after being freed from their lower attachments, the knee-jerk, previously brisk, was at once *abolished or greatly diminished*, and on relaxing the strain on the hamstring muscles at once reappeared, and was apparently somewhat more brisk than before the diminution. It is often sufficient to merely compress the hamstring muscles, as they lie flaccid on the hand, between fingers and thumb. A kneading of the muscle as in *massage* has the same effect. On two occasions, at the end of an experiment, when the muscles had suffered from exposure, I have seen the curious phenomenon that excitation of a motor root supplying them of strength insufficient to throw the injured muscles into obvious contraction yet suffices to at once cut out the "knee-jerk," although before and after the excitation the "jerk" was very brisk indeed. The effect was obtained several times in succession, and immediately disappeared on dividing the sensory roots coming in from the exposed muscles. The current of injury in the muscles must have been considerable, and this suggests that the mere negative variation of the current of injury in the muscles might originate centripetal impulses. Certainly there was on neither occasion any obvious contraction in the muscles.

The most efficient mode of excitation of the afferent fibres from these muscles appears to be the mechanical above described, *i.e.*, the *myotatic* (Gowers).*

Excitation of the central end of the divided popliteal or peroneal nerves does not produce this effect upon the jerk. Neither does stretching of the crural triceps by pulling on the tendo Achillis, nor stretching of the rectus femoris muscle. Stretching of the rectus femoris can easily be employed without interference to the movement of the knee joint. It appears to me neither to increase nor to diminish the jerk. Excitation of the central end of the divided nerve to the rectus femoris exerts likewise no obvious influence on the jerk; nor does excitation of the central end of the cutaneous divisions of the anterior crural, viz., the internal saphenous and the internal and middle cutaneous nerves of the thigh. Excitation of the central

* 'Diagnosis of Diseases of the Spinal Cord,' 2nd edit., 1881, p. 29. See also the same author's "Diseases of the Spinal Cord,' vol. i, p. 21; also pp. 202—205, 428, 429, 2nd edit., 1892.

end of the cutaneous branch of the hamstring nerve itself also appears without effect upon the jerk.

Tension produced in the crural triceps muscle by pulling on the Achilles tendon does not appear to influence the brisk flexor movement of the ankle joint evoked by tapping the subcutaneous face of the tibia, and related chiefly to the 6th lumbar spinal segment of *Macacus* and to the 7th lumbar segment of the Cat.

It would thus seem clear that the exaggeration of the knee-jerk produced by severance of the branches given from the great sciatic nerve to the hamstring muscles is not due to the fact that the resulting relaxation of those muscles simply leaves the joint mechanically more free to move. The exaggeration would seem due rather to the severance of the nerves in question interrupting a stream of centripetal impulses that passes up from the hamstring muscles and enters the spinal cord by certain afferent roots, and in the cord exerts a depressing or restraining influence on the jerk. It further appears that a stream of impulses similarly efficient can be set up by moderate electrical excitation of the central ends of the divided nerves of the hamstring muscles; or by simply stretching or kneading those muscles when they are released from one of their fixed points; or, finally, by simply throwing those muscles into contraction through excitation of motor roots supplying them, so long as the sensory roots remain intact. The physician, when he, in order the better to elicit the jerk, flexes the knee and reduces the strain in the flexor muscles, by doing so removes, with relaxation of the flexors, a physiological depression which the tension of those muscles normally exerts upon the jerk obtainable from their antagonistic group.

Further, it would seem that at the knee joint excitation of the afferent fibres coming from one set of the antagonistic muscles induces reflex tonic contraction of the opposing set with extreme facility, despite the fact that the opponent muscles are not innervated from the same spinal segments. The reflex is obtainable with extraordinary facility, even across intervening segments of the cord.

Thus the degree of tension in one muscle of an antagonistic couple intimately affects the degree of "tonus" in its opponent, not only mechanically, but also reflexly, through afferent and efferent channels and the spinal cord.

It is obvious that the correlation of action thus existing between the antagonistic muscular groups of the thigh and knee may be not widely different from that originally pointed out by Hering and Breuer as regulative of the movements of respiration. One is tempted to institute a comparison also between it and the physiological arrangement studied by Biedermann in the antagonism of the muscles of the forceps of *Astacus*. I would, however, reserve further details until I have been able to perform a larger number of

experiments. Anatomical evidence is at present so scanty regarding afferent nerve fibres from muscle that investigation of their anatomical relation seems absolutely requisite for examining the problem further.

[Just as the lumbo-sacral region of the cord may be split along the median plane without interference to the jerk of either side,* so the same may be done without hindering the above ascending reflex abolition of the jerk. Extinction of the jerk by exciting the central end of the 8th root (from hamstrings) affects the jerk four segments higher without in that distance spreading over to the opposite side. But the excitation affects the jerk of the opposite side if the scope of a considerable length of cord be allowed it. If in the Cat the cord be transversely divided at the 11th thoracic segment, excitation of the afferent fibres from a hamstring muscle of one side (*e.g.*, right) applies chiefly to the jerk on the same side (right), but also to the jerk on the opposite. If, however, in the Cat (in which jerk belongs to the 6th and 5th lumbar segments) the cord be transversely cut at or below the 3rd, the extinction from the hamstring nerve is confined to the same side only. In other words, the presence of additional higher segments seems requisite before the passage of the impulses in question across the median plane of the cord, a fact in curious harmony with an observation by Hallstén† regarding the elicitation of "crossed reflexes" in the frog. The median posterior column between the 8th and 4th lumbar levels can be removed *in toto* without impairing the influence of the hamstring nerve on the jerk. It is clear also that those fibres of the posterior root which pass to Clarke's column cannot be the requisite afferents, either from the extensor or flexor thigh muscles, because the jerk and the above-described extinction of it are unaffected in the Cat by transverse section of the cord just below the 4th lumbar segment, *i.e.*, the segment where Clarke's column stops short.—*February* 8, 1893.]

III. "On the Leucocytes of Peptone and other Varieties of Liquid Extravascular Blood." By A. E. Wright, M.D., Professor of Pathology, Army Medical School, Netley. Communicated by A. D. Waller, M.D., F.R.S. Received January 30, 1893.

In the course of some investigations on the subject of blood-coagulation, I was led to enumerate the white blood corpuscles in the different varieties of extravascular blood. I propose to report the

* Sherrington, 'Journ. of Physiol.,' vol. 13, p. 666.

† 'Archiv f. Physiol.,' 1885.

results of these enumerations here, and to direct attention to the bearing of the results obtained in the case of peptone blood.

I may premise with respect to the methods employed that the enumerations were made by diluting the blood 200 times with an 8 per cent. solution of magnesium sulphate, which had received a sufficient addition of gentian-violet to stain the cells darkly in the course of a few minutes. The enumerations were invariably made in duplicate samples of each blood, and in each sample the absolute number of leucocytes was counted on 250 squares of the Gowers hæmocytometer. An agreement within 10 per cent. was exacted between the counts of the duplicate samples. When this was obtained the counts were added together, and their sum multiplied by 200 to obtain the total number of leucocytes in the cubic millimetre. The following are the results of my enumerations in oxalated, leech-extract, and peptone blood.

Oxalated Blood.

Obtained by drawing off 9 vols. of blood into 1 vol. of a 1 per cent. oxalate of soda solution.

	Number of white blood corpuscles in normal blood drawn from ear.	Number of white blood corpuscles in blood received from carotid into oxalate solution (figures corrected for dilution effected by the oxalate solution).	Interval between collection of blood into oxalate and commencement of enumeration (*i.e.*, dilution with $MgSO_4$ solution).
Dog 1	15,600	15,800	One hour.
Dog 2	10,400	11,200	Half an hour.
Dog 7	20,500	19,700	No interval.
Dog 8	14,000	14,600	Two hours.
Dog 14	22,300	20,600	One hour.
Sum	82,800	81,900	
Average	16,560	16,380	

Leech-extract Blood.

Obtained by extracting 3 vols. of blood from carotid into 2 vols. of leech-extract. The leech-extract was made from leeches which had been kept under alcohol for several weeks.

	Number of white blood corpuscles in normal blood drawn from ear.	Number of white blood corpuscles in blood received from carotid into leech extract (figures corrected for dilution effected by leech-extract).	Interval between collection of blood and commencement of enumeration (dilution with $MgSO_4$ solution).
Dog 8	14,000	13,100	Nearly two hours.
Dog 13	16,500	14,250	One hour.
Dog 14	22,300	20 000	Three-quarters of an hour.
Dog 15	12,600	12,500	Quarter of an hour.
Sum	65,400	59,850.	
Average	16,350	14,962	

Peptone Blood.

Obtained by injection of 0·3 to 0·5 gram per kilo. of body weight of "peptone" in the form of a filtered 10 per cent. solution in 0·75 per cent. of NaCl.

	Number of white blood corpuscles in normal blood drawn from ear.	Number of white blood corpuscles in peptone blood drawn from carotid.	Interval between peptone injection and blood-letting.	Interval between collection of blood and commencement of enumeration (*i.e.*, dilution with $MgSO_4$).
Dog 1....	..	2,500	10 minutes	One hour.
Dog 6 ...	30,600	3,200	8 "	"
Dog 8....	14,000	600	5 "	"
Dog 9....	19,600	1,800	Not noted	Not noted.
Dog 10...	..	2,800	10 minutes	No interval.
Dog 14...	22,300	1,300	2 "	"
Dog 20...	14 800	1,600	5 "	"
Dog 21...	10,800	600	10 "	"
Dog 22...	13,800	800	25 "	"
Dog 23...	15,900	400	10 "	"
Sum.....	8)141,800	10)15,600		
Average..	17,725	1,560		
Rabbit 1.	7,400	3,600	15 minutes	No interval.
		750	3½ hours	"
Rabbit 2.	10,000	9,200	30 minutes	"
Rabbit 3.	8,000	4,600	2 hours	"
		3,300	3 "	"
Rabbit 4.	8,600	4,100	30 minutes	"
		2,500	3 hours	"
		2,800	3½ "	"

The figures in column 2 have not been corrected for the dilution effected by the injection of the peptone solution. Taking the blood as 1/13 of the body weight the amount of peptone solution introduced would effect a dilution of from 3·9 to 6·5 per cent. The figures in column 2 ought therefore to be increased by this fraction in order to be strictly comparable with the figures in column 1.

The above tables show that Dog's peptone blood differs from oxalated, leech-extract, and 8 per cent. $MgSO_4$ blood (this last blood was employed throughout as the standard of comparison) in containing a mere tithe of the normal number of leucocytes. These missing leucocytes have either remained behind in the tissues or in the internal vessels, or they have disintegrated and have passed into solution in the plasma. I have endeavoured to decide between these alternatives by making comparative estimations of the leucocytes in the mesenteric veins, and in the carotid blood, and further by making a series of careful histological examinations of the various organs which might be expected to harbour the leucocytes (I selected the liver, the kidney, and the heart muscle for this purpose). In no case was I able to find any trace of stasis or of emigration of leucocytes either in the Dog or in the Rabbit after peptone injections. I, therefore, feel justified in concluding that in all probability the leucocytes have dissolved in the plasma. I believe that this view is borne out also by a consideration of the chemical properties of peptone plasma, notably by the fact that it deposits on cooling a heavy precipitate of a nucleo-albumen, which is probably identical* with Wooldridge's tissue or cell-fibrinogen, in other words, identical with the characteristic albuminous constituent of the white blood corpuscle.

This "cold precipitate" is not obtained from any other plasma except from oxalate plasma, where I have obtained it, after allowing it to stand for 24 hours before separating it from the white blood corpuscles. Under these circumstances a certain disintegration of white blood corpuscles takes place in this plasma. The non-occurrence† of a "cold precipitate" in leech-extract plasma is in accordance with the fact that this plasma contains no disintegrated leucocytes. The non-occurrence of the precipitate in salted plasmas (Halliburton) probably similarly depends on the fact that the white blood corpuscles do not disintegrate readily in these plasmas, but it may be noted that Wooldridge showed that the addition of neutral salts prevented the precipitation of his "cold precipitate."

We have thus reason to believe that the occurrence of a precipitate on cooling peptone plasma is due to the fact that the plasma contains

* *Vide* Pekelharing's identification of Wooldridge's "cold precipitate," or "A-fibrinogen," with the nucleo-albumen of the cell ('Verhandl. d. Konink. Akad. v. Wetenschappen, Amsterdam,' 2nd Sect., Deel 1, No. 3).

† See Dickinson, 'Journ. of Physiol.,' vol. 11.

leucocytes in solution. If this is so, we have a ready explanation of some of the other characteristics of peptone plasma, notably of the fact that the CO_2 in this blood is remarkably diminished as compared with the normal blood, and also of the fact that peptone plasma clots when a stream of CO_2 is passed through it.

With regard to the diminution of the CO_2 in peptone blood, Lahousse (Du Bois-Reymond's 'Archiv,' 1889) surmised that it was due to a driving out of gas from the blood. He based this view on the extreme rapidity with which this diminution occurred in the blood after peptone injection. Blachstein (Du Bois-Reymond's 'Archiv,' 1891), who followed up Lahousse's work, contributed the following to our knowledge of the question. A diminution of CO_2 is found in the Rabbit's blood as well as in the Dog's blood after the injection of peptone. In the three experiments reported by Blachstein the CO_2 of the normal Rabbit's blood stood to the CO_2 of the peptonised Rabbit's blood approximately in the relation of 4 : 3, 4 : 3, and 3 : 2. In his three experiments on Dog's blood the ratios were approximately 3 : 1, 3 : 1, and 2 : 1. It will be noticed that the CO_2 undergoes a greater diminution in Dog's peptone blood than it does in Rabbit's peptone blood.

Grandis (Du Bois-Reymond's 'Archiv,' 1891) pursued the subject further, and demonstrated that the tension of CO_2 in peptone blood is approximately double that of the CO_2 in normal blood. He indicates that the phenomena point clearly to the liberation of some substance with acid properties in the blood.

In view of these facts I would suggest that this substance with acid properties is in all probability the nucleo-albumen of the white blood corpuscles which have become dissolved in the plasma under the influence of the peptone injection. The liberation of this substance in the blood would result in a driving out of CO_2 from its combination with the bases of the blood plasma, and would thus account for the great diminution of the CO_2 in peptone blood. The differences in this respect between Dog's and Rabbit's peptone blood are in perfect agreement with the results of the enumerations given above for those bloods. The hypothesis of the driving out of CO_2 by a liberation of nucleo-albumen in the blood would further harmonise with the increase of the tension of the CO_2 in peptone blood, and also with the diminished excretion of CO_2 after peptone injections (Bohr, 'Centralblatt f. Physiol.,' 1888).

The fact that casein (better, perhaps, called "caseinogen," Halliburton) will drive out CO_2 from $CaCO_3$ constitutes an almost perfect analogy with the property of driving out CO_2 which is here surmised to characterise Wooldridge's cell-fibrinogen. In both cases we are dealing with nucleo-albumens.

With respect to the coagulation which is produced in peptone

plasma by passing a stream of CO_2* through it, it appears to me that this might be very naturally explained by assuming that we are dealing with a direct reversal of the process which occurs when peptone is injected into the blood. The CO_2 coagulation in peptone blood would upon this hypothesis be due to a precipitation of cell-fibrinogen in the plasma under the influence of an excess of weak acid. Such a precipitation of cell-fibrinogen in the plasma would be in some sort an equivalent of an addition of cell-fibrinogen to peptone plasma, and would, therefore, naturally inaugurate coagulation.

The coagulation of peptone plasma *in vitro* by CO_2 would be closely paralleled by the fact that intravascular coagulation after injection of cell-fibrinogen occurs, as I have pointed out ('Journ. of Physiol.,' 1890), only in the vascular areas where CO_2 is present in excess.

The precipitation of cell-fibrinogen in peptone plasma under the influence of CO_2 would further have a close analogy in the precipitation of its congener caseinogen from diluted milk by the addition of excess of dilute acids.

Presents, February 9, 1893.

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* This coagulation does not occur in any other plasma; neither does it occur in blood which has been kept liquid by an addition of peptone *in vitro*. It may be noted that the addition of even 8 per cent. of peptone to blood *in vitro* does not entail any destruction of leucocytes.

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OBITUARY NOTICES OF FELLOWS DECEASED.

Sir William Bowman was born at Nantwich in 1816. His mind was in early life directed towards natural science; for his father, Mr. John Eddowes Bowman, a banker, was a Fellow of the Linnean Society, an ardent botanist and geologist, an intimate associate with many of the best naturalists of his time, and, during the last years of his life, one of the most active promoters of literary and scientific societies in Manchester. The 'Catalogue of Scientific Papers' gives references to fifteen papers on various subjects which were published by him between 1828 and 1842 in the Transactions of the Linnean and Geological Societies or in scientific journals. They show that he was a careful and minute observer in a wide range of study, a clear and vigorous descriptive writer, and a good artist; the illustrations of some of his papers, engraved from his own drawings, are admirable.

Sir William in his boyhood studied botany and geology with his father and drawing with both his parents; for his mother also was an accurate and skilful artist *ad naturam*, both with the pencil and in needlework. A disposition for earnest study and for high mental and moral culture prevailed in his home, and all his brothers were in their several ranges of work successful and well esteemed. One was Professor of Classics in the Manchester New College; another was an architect of good repute in the same town and author, with his partner, of a work on "The Churches of the Middle Ages;" the third was Professor of Practical Chemistry in King's College, and author of two small widely-read books on Practical Chemistry and on Medical Chemistry. They all died before him.

After completing his general education at Hazelwood School, near Birmingham, Bowman was apprenticed to Mr. Joseph Hodgson, a Fellow of the Royal Society, Surgeon to the Birmingham General Hospital, and one of the most distinguished surgeons of the time. He could not have had better opportunities for the study of surgery than were thus provided for him. The hospital, in which he was a house-pupil, supplied large numbers of cases which he could observe and record, and Mr. Hodgson was not only an able teacher and an earnest worker in surgery, but, as is proved by the illustrations of his book on 'Diseases of the Arteries,' which were engraved from his

own drawings, he was an excellent anatomical artist. Thus encouraged, Bowman studied constantly and carefully, and made many records of cases and pathological drawings, some of which were published in 1834, in illustration of Ryland's 'Essay on Diseases of the Larynx and Trachea.' The records of the cases and the coloured engravings appear alike accurate, and are at least remarkable as the work of a student only eighteen years old.

In 1837 he entered as a pupil at King's College, and, after the course of study usual at that time, became a Member of the College of Surgeons in 1839. In the same year he was appointed a Demonstrator of Anatomy and Curator of the Museum at King's College, and he gave himself especially to the study of minute anatomy. The improvements of microscopes and the facilities of working with them had, at that time, so increased the range and importance of histology (or general anatomy as it was then called), that it had become the most attractive of all the subjects of biological research. It was only in 1836, on the appointment of Dr. Sharpey at University College, that it was, for the first time in any London school, separated from the teaching of descriptive anatomy and combined with that of physiology. Dr. Todd, soon after, taught it in the same method at King's College, and Bowman worked with him and for him. Dexterous, patient, loving completeness, and working after the manners of both the naturalist and the artist, he soon attained great results, and they were shown, especially, in four papers, illustrated with engravings from his own drawings, which were published in 1840 and the following two years. The first, "On the Structure and Movements of Voluntary Muscles," was published in the 'Philosophical Transactions' for 1840. It was followed, in the next volume, by an "Additional Note on the Contraction of Voluntary Muscles"; and in the volume for 1842 by a paper "On the Structure and Use of the Malpighian Bodies of the Kidney"; and a paper "On the Minute Anatomy of Fatty Degeneration of the Liver," was published in the 'Lancet,' of January 22, 1842. Among the speedy evidences of the value of these papers were his election into the Royal Society in 1841, and the award of a Royal Medal in 1842; but yet more complete evidence was and is in their abiding reputation. Other anatomical essays of great value were published by him between 1842 and 1851. They include the articles on Mucous Membrane, Muscle, Muscular Motion, and Pacinian Bodies in the 'Cyclopædia of Anatomy and Physiology,' vol. 3; eight others relating chiefly to muscles and the sense-organs, in the 'Physiological Anatomy and Physiology of Man,' which was published between 1843 and 1856, and of which Dr. Todd and himself were joint authors: and a small volume of "Lectures on the parts concerned in the Operations on the Eye and on the Structure of the Retina," which were given at the Royal Ophthalmic Hospital in 1847, and with which

were included a notable paper on the Structure of the Vitreous Humour, and a few Cases of Ophthalmic Disease.

It may justly be believed that no series of papers by one author has been more important in the progress of histology and of its influence on physiology than were these. They have been called by many "epoch-making;" and the term may certainly be well applied to those on the structure of muscles and of the kidneys.

In the structure of striated muscle, Bowman showed that the whole substance of the fibre, "primitive fasciculus," exclusive of the sarcolemma and nuclei, "muscle-corpuscles" (of both of which he gave the first minutest account), was composed of particles, "sarcous elements," united together by an intermediate material (concerning which he offered no exact opinion), so as to form in one direction fibrillæ, and in another discs. This conception cleared away the many previous views as to the nature of the striation; and, though subsequent researches have led to the belief that the structure of the fibre is more complicated than he thought, yet, in many ways, the ideas at present accepted may be regarded as expansions or modifications of those expressed in these papers. The "sarcous element" is not now regarded as a simple homogeneous body, but as one made up of different parts: but it is still held that the fibre is made up of such "elements," and that, as Bowman believed, the act of muscular contraction consists essentially in a change in these elements.

By his researches on the structures of the kidney he first made known the relation of the blood-glomerules to the uriniferous tubules, which were previously supposed to end blindly or in loops, with the glomerules lying in spaces outside them. He gave a very good account of the circulation in the kidney, and rendered possible a true view of the process of urinary secretion, and suggested the double nature of that process; a suggestion which subsequent researches have done little more than amplify and enforce.

In his work on mucous membranes, Bowman gave clearness and distinctness to the views previously arrived at by Henle; and by his descriptions of their structures and of the glands associated with them, he contributed largely to the knowledge of the process of secretion. And in his studies of the structures of the eye he added clearness and fulness to all previous descriptions of them, and made many things sure that were before his work deemed doubtful. Indeed, it may safely be said that scarcely inferior in value to his discoveries were the many instances in which his accuracy and clearness decided doubts and questions, and led to the general acceptance of his descriptions as sure for teaching and for guidance in further research. And, though he did not often work at what might be deemed experimental physiology, yet he derived from his minute anatomy of structures many accurate suggestions of their modes of action; he called it

Physiological Anatomy, and made it distinctly physiological whenever it was reasonable to deduce function from structure.

In 1848 Bowman was appointed joint lecturer with Dr. Todd on Physiology and Physiological and Morbid Anatomy at King's College, and this appointment he held till 1855, when he was succeeded by Dr. Beale, who had shared the lectureship with him in and after 1853. His lectures were, like his published works, clear, well-defined, plainly descriptive, fully illustrated by specimens and drawings; and he lectured very earnestly, as one sure of the importance of his subject, always at work in it, and wanting his pupils to work with him. His resignation of his lectureship was due, chiefly to his constantly increasing practice; for, busy as he had been in scientific research, he had never neglected surgery. In 1840, when King's College was established, he and Mr. (now Sir John) Simon, who had been Senior Demonstrator of Anatomy, were appointed Assistant Surgeons, and in 1846 he was appointed to a similar office in the Royal London Ophthalmic Hospital in Moorfields. In both these he gained high repute, for he was as observant in the practice of surgery as he was in the study of structures, and he was very dexterous and careful. It was for some time doubtful whether general or ophthalmic surgery would be his chief occupation; but this was determined by events over which he had no control. At the time of his appointment at Moorfields, in 1846, Mr. Dalrymple, who was the Chief Surgeon to the hospital, had the leading ophthalmic practice in London, but his health was failing, and when he died, in 1851, Bowman succeeded him both in the hospital and in the chief private practice. From this time he gave himself almost entirely to ophthalmic surgery, and his devotion to it was increased by the singular coincidence that, in 1851, while he was at the hospital, von Graefe and Donders met there for the first time. They both worked with him, studying especially with the recently invented ophthalmoscope; and thus began their life-long friendship and constant mutual help and encouragement. His last published writing was his biography of Donders in the 49th volume of the Proceedings of this Society.

Bowman was singularly well fitted for ophthalmic surgery: light-handed, dexterous, clear-sighted, calm, and, by his own researches, familiar with all the structures of the eye, his operations were faultless. His educated sense of sight made him perfect in the use of the ophthalmoscope, and his habits of minute research made him observant of every thing that might guide to accurate diagnosis. Besides, he had many habits and mental qualities favourable to the attainment of success. He was punctual, businesslike, calm and deliberate in consultation, gentle and sympathetic, always clear in the statement of his opinion, making all feel confidence in him. And he never showed any of the faults often ascribed to specialists. His method of work

continued, as it always had been, thoroughly scientific; all the papers that he published show the same care and completeness, the same earnestness and width in research as his earlier anatomical studies; and he never lost the advantage of the general knowledge of surgery which he gained in his duties at King's College Hospital, to which, after sixteen years' service as Assistant Surgeon, he was appointed Surgeon in 1856.

Of the numerous ophthalmological papers which he published after 1850, and which are all included in the second volume of his collected works, the most important and influential on practice were those on the Treatment of Epiphora, in the 'Medico-Chirurgical Transactions,' vol. 34, and in the 'Ophthalmic Hospital Reports,' vol. 1, 1857; on Conical Cornea, in the same Reports in 1859; and on the use of two needles in Capsular Cataract, in the same Transactions, vol. 36. Other valuable papers were published in the 'Lancet' and other medical journals, and in the 'Proceedings of the Ophthalmological Society;' and of all it may justly be said, as it may of his minute anatomical work, that, although later studies of the same subjects have added facts and have increased the practical utility of those which he had made known, yet none have detected in them errors. In practice, as in minute research, he was always accurate and careful, and limited his deductions to such as he could, at least, be nearly sure of. He thus exercised great influence in the increase and diffusion of the knowledge of safe ophthalmic surgery, and scarcely less than the influence of his writings was that which was due to his speedy adoption of the right opinions and practices of others, especially in the use of the ophthalmoscope, and of the antiseptic treatment of wounds, and in the promotion of von Graefe's pathology and treatment of glaucoma, and of Donders's discoveries relating to errors of refraction and their remedies.

The quantity of work that Bowman did may appear the more remarkable for the fact that his health, though not unsound, was never vigorous. It was, probably, for this reason that he limited his work almost exclusively to study and the plain duties of his life. In subjects far beyond his range of work he often seemed to enjoy thinking and wondering, but he did not give his whole mind to them; he worked for facts, and for the best use of facts in his practice or in the extension of science. Similarly, he avoided all the larger official and social occupations of public and professional life, and engaged only in some that were nearly associated with his proper work, as in the Councils of the Royal Society and of King's College, in the Royal Institution, in the Ophthalmological Society, of which in 1880 he was elected the first President, and in the Association for the Advancement of Medicine by Research, of which he might be regarded as the founder. In these he did all that could be deemed his duty, and he

did it quietly, wisely, and without self-assertion; just as he did many generous things in charity and the promotion of religion.

His personal character, hardly less than his scientific eminence, made all who knew him wish to show him how widely and deeply he was esteemed. He received many honours from universities and scientific societies, the titles of which are added to his name in the List of Fellows; he was created a baronet in 1884; his friends at home and abroad presented him with his portrait by Mr. Ouless, R.A., and arranged for the reprinting of all his works and all their illustrations in the complete edition recently edited by Professor Burdon Sanderson and Mr. Hulke, after careful revision by himself; and in 1883 the Ophthalmological Society founded in his honour the "Bowman Lecture."

He had resigned the Surgeoncy of the Ophthalmic Hospital in 1876; but he retained the pre-eminence in private practice till, after holding it for more than thirty years, he began to retire and to spend an increasing portion of his time at Joldwynds, near Dorking, where he had a country-house in beautiful scenery, and gardens which he loved to cultivate. Here he enjoyed the prosperity which he had attained after great difficulties in his early professional life, and with it he had complete domestic happiness. He had married in 1842 a daughter of Mr. Thomas Paget, a highly esteemed surgeon at Leicester; they had seven children, and, as he had imitated his parents in his scientific and artistic studies, so, and with the like success, were they imitated in his home. As one who could speak with knowledge states, "they were imitated in the high moral and intellectual training of the children," and "in the high standard of rectitude which they maintained." But, besides all this, he enjoyed his partial retirement because it gave him time to gratify his love of Nature and of scientific observation. He cultivated rare plants, and those about which questions might, perhaps, be determined by experiment; he set apart a piece of land in which he might find the fittest place for each species, and subject it to various tests; and, with willing coincidence, he especially studied the Lathrææ, which had been the subject of one of his father's earliest investigations. He formed what Mr. Thiselton Dyer, with whom he was in frequent correspondence, and from whom these facts were learned, considered one of the best collections of open-air plants in the country, and he often blended his scientific work with his love of art in so grouping shrubs and flowers as to show by the arrangement of forms and colours the most perfect beauty. Thus he attained old age without apparent diminution of mental power or of the love of acquiring knowledge; and, to the last, his studies of science and of art gave him great happiness. It was little more than a week after his final retirement from practice, and his leaving the house in Clifford Street in which he had long lived,

that, on the 29th of March in last year, he died of pneumonia at Joldwynds.

J. P.

Carl Schorlemmer, Professor of Organic Chemistry in Victoria University, Owens College, Manchester, was born in 1834, at Darmstadt, a town remarkable as having given birth to many distinguished chemists. He was educated at Darmstadt and at Giessen; he came to Manchester as private assistant to Professor Roscoe, in the year 1858, and shortly afterwards, on the resignation of Mr. Dittmar, he became the official laboratory assistant in Owens College. In this position he not only supervised the practical work of the laboratory, but also lectured on organic chemistry; yet he found time for original investigation, and shortly afterwards began his work on the hydrocarbons, which has placed his name high in the list of organic chemists of the century. The commencement of this research was due to the initiative of the late Mr. John Barrow, of the Dalton Chemical Works, Gorton, who placed a sample of the light oils which he had obtained in the distillation of cannel coal at Professor Roscoe's disposal. At that time our knowledge of the chemical composition of the light boiling coal-oils was very incomplete, and Schorlemmer was urged to begin the investigation.

This work led to results which altogether modified the existing ideas concerning the constitution of the paraffin hydrocarbons, and laid the foundation of the modern science of organic chemistry.

Up to 1848 the only known member of the paraffin series of hydrocarbons was methane, CH_4. In the above year the researches of Kolbe, on the one hand, on the electrolysis of the fatty acids, and those of Frankland, on the other, respecting the isolation of the so-called alcohol radicals, had opened out new fields for investigation.

The hydrocarbons, then termed the alcohol radicals, were supposed to contain two molecules of the radical, methyl being represented as $CH_3—CH_3$, ethyl as $C_2H_5—C_2H_5$. Together with these, a second series of hydrocarbons, possessing the same composition, was supposed to exist, thus: C_2H_5H, ethyl hydride, having the same composition as the radical methyl, and C_4H_9H, butyl hydride, having the same composition as the radical ethyl, were considered as being bodies standing in the same relation to the radical as an alcohol does to an ether.

The existence of two such series of hydrocarbons is, however, only explicable upon the supposition of a difference existing between the four combining powers of each carbon atom, and such a supposition is at variance with a general theory of the mode of building up of the carbon compounds.

Schorlemmer at once saw the importance of the question, and not only seized upon the correct method of solution, but carried it out successfully. If, said he, the radical methyl, $CH_3—CH_3$, is identical with ethyl hydride, C_2H_5H, not only must they have the same properties, but must yield the same product on treatment with chlorine.

This identity he proved, experimentally, not only in the above, the most simple instances, but also in the more complicated cases of ethyl-amyl and di-amyl, $C_2H_5—C_5H_{11}$ and $C_5H_{11}—C_5H_{11}$, these hydrocarbons being shown to yield, respectively, chloride of heptyl and chloride of decatyl, $C_7H_{15}Cl$ and $C_{10}H_{21}Cl$. It is difficult to overestimate the importance of this discovery, for it proved the non-existence of two sets of isomeric hydrocarbons of the paraffin series, and, therefore, placed the theory of carbon combination on a firm basis. This, though the most important of Schorlemmer's researches, is only one amongst many, a list of thirty-two separate papers being found in the Royal Society's Catalogue. In 1871 he was elected a Fellow of the Royal Society, and in 1874 his position in the scientific world was acknowledged by the creation by the Council of Owens College of a distinct Chair of Organic Chemistry, the only one at present in England, of which he was the first occupant.

It was, however, not only as an expert experimentalist that Schorlemmer excelled. He possessed an exhaustive knowledge, uncommon even amongst professional chemists, of the extensive literature, not only of the chemical science of his day, but also of that from the earliest times.

His acquaintance with other subjects was also considerable; thus, if he had not been a distinguished chemist, he would have been an equally remarkable botanist. Moreover, he possessed, in full measure, that dogged power of work which distinguishes the German, and which has placed that people in the front as a nation of investigators.

He became joint author with Professor Roscoe in the 'Treatise of Chemistry,' and in the organic portion of that work especially, he has shown both a perfect knowledge of the science, and remarkable literary power. Only those who have attempted to write, even a moderately complete treatise on modern organic chemistry, can know what serious labour such work entails. Several distinguished chemists have given up the task as hopeless, and have not completed what they had begun. If Schorlemmer's life had been spared, nothing would have prevented him from bringing his work to a satisfactory conclusion.

As a laboratory teacher Schorlemmer was excelled by few, merely as a lecturer by many. But, although, like some eminent lecturers, his diction may have been faulty, the staple article was there, and no real student who passed through his hands can be named who does not express his admiration for the man, and the sense of obligation

which he felt for the masterly instruction which he received. The long list of honours which his students gained, both in London, and afterwards in the Victoria University, prove how successful his teaching was.

Although for many years a naturalised Englishman, and enjoying and appreciating English freedom and English ways, he retained more than is usual a lively interest in the welfare of the "Vaterland."

Schorlemmer's name will long be remembered in the annals of our science as an investigator, as an author, and as a teacher.

H. E. R.

Henry Tibbats Stainton, who died at Mountsfield, Lewisham, on December 2, 1892, was the eldest son of Henry Stainton, Esq., of Lewisham. He was born in London on August 13, 1822, but was removed to Lewisham when only a few weeks old, and thus practically resided all his life in this now suburban borough. He was educated almost entirely at home, and by a lady, and finally for a short time at King's College, where amongst his companions was Professor Cayley. The regulations of his father's household were very stringent, and young Stainton during the whole of his childhood and boyhood was not allowed to mix with companions of his own age, and to this, and partly perhaps also to extreme myopia, the marked diffidence, amounting almost to shyness, exhibited by him throughout life was probably due. For several years he was engaged in commercial occupations under his father, and thus acquired those habits of method and accuracy that were so marked in his character during the whole of his career, for he never wrote a letter to which there was the slightest possibility attached of reference to it being necessary hereafter without hand copying it. About the year 1840 he made the acquaintance of the Rev. William Johnson, a friend of his father, and an ardent collector of British Lepidoptera, and it was no doubt from him that he acquired a taste for entomological pursuits, to which he at once devoted himself with great ardour. From the first his attention as a specialist turned to the small Moths known as Micro-Lepidoptera, and one of his first essays was "A Monograph of the British Argyromiges," published in the 'Zoologist' for 1848, a work which he was in after years wont to style "most rubbishy." At the commencement of his scientific career Stainton discovered that British entomologists were not in touch with their fellow-workers on the Continent and elsewhere, and he commenced a correspondence with all the prominent students of his special branch, which as years went on was strengthened by visiting them personally and by inviting them to be his guests at Mountsfield. After the death of James Francis

Stephens, in 1852, he acquired the very extensive library formed by that entomologist, and built an annexe to his house for its reception, and this formed the nucleus of the rich collection of entomological works of which he ultimately became possessed.

Stainton's forte undoubtedly lay in the direction of working out the life histories of the minute Insects he studied. When in early life other occupations gave him but little of an ordinary working day to devote to his studies, he got over the difficulty by rising at five o'clock in the morning, and this habit (which was partly inherited) he continued down to the beginning of his fatal illness. The minute size and the concealed habits of the majority of the objects of his study rendered the greatest patience necessary for the discovery of their modes of life, and the peculiar condition of his eyesight (before alluded to) no doubt aided him, for by the unaided eye he was able to detect what to most others was invisible without a lens, and it was probably owing to this cause that during the whole of his life he was never able to use the compound microscope, but contented himself with a pocket glass of comparatively low power. His principal separate works are, 'An Attempt at a Systematic Catalogue of the British Tineidæ and Pterophoridæ,' 1849, with Supplement in 1851; the volume on "Tineina" in the 'Insecta Britannica' series, 1854, which from a strictly scientific point of view is usually regarded as his best; 'A Manual of British Butterflies and Moths,' 2 vols., 1857 and 1859, which is still the most concise treatise on the subject; 'The Natural History of the Tineinæ,' in 13 vols., 1855–73, with the assistance of his old friend Mr. J. W. Douglas, and of Professor Zeller, of Glogau (afterwards Stettin), and Professor Frey, of Zürich, an ambitious work in four languages, each volume delineating and describing the life histories of a large number of species, and for which a host of still unpublished material had been accumulated; several local faunæ, such as the 'Tineina of Southern Europe' (in connexion with which he passed each spring for many years upon the Riviera); 'Tineina of Syria and Asia Minor'; and 'Tineina of North America' (the latter edited by him). He also published many papers in the Transactions of Societies and in Journals, and in connexion with entomological journalistic literature Stainton became most prominent. In 1856 he established the 'Entomologist's Weekly Intelligencer,' published at one penny (!), an attempt to popularise the study in this country, with, at the same time, a view to induce mere collectors to look at the subject from a more serious point of view. If not always successful, he, through the medium of this journal, was the means of bringing forward many young men who afterwards became prominent workers. The 'Intelligencer' went through ten volumes, and was discontinued in 1861. The 'Entomologist's Annual' was commenced in 1855 and continued

to 1874, mainly a *résumé* of the additions to the British Insect fauna in each year. In 1864 he, in conjunction with several friends (of whom the writer of this notice was one), established the 'Entomologist's Monthly Magazine,' which continues, and of which he looked over the revise of the number for December, 1892, only a few days before his death. For many years he held *réunions* of young entomologists at his house, for a long time every Wednesday evening, and subsequently less frequently; and it was on one of these occasions that the writer first met him, about thirty-five years ago, an event that ripened into a friendship for life.

In 1848 he joined the Entomological Society of London, was a Secretary in 1850–51 and President in 1881–82, and there were few more regular attendants at the meetings. He was elected into the Linnean Society in 1859, was a Secretary for a short time, and a Vice-President in 1883–85. He became F.R.S. in 1867, and was on the Council in 1880–82. At the meetings of the British Association for the Advancement of Science he acted as a Secretary of Section D in 1864, and from 1867 to 1872. He became Secretary of the Ray Society in 1861 at a critical moment in its history, and continued down to 1872. In 1871 he was instrumental in founding the 'Zoological Record' Association, of which he became Secretary until the publication was taken over by the Zoological Society in 1886, and to his liberality it was largely owing that the 'Record' was not allowed to lapse.

The indications given above show some of the results (but by no means all) of an unusually busy life; in fact Stainton revelled in work. He was never of a strong constitution, and suffered much from gastric troubles. It must be now fully fifteen years ago that he had a serious illness, but not of long duration. Nevertheless those who knew him intimately noticed a marked change in his character afterwards. His capacity for close continuous study became impaired, and he sought work more and more in a secretarial and administrative direction.

Stainton was a man of great geniality, and also of most marked individuality, extremely methodical and conservative in his habits, slow to accept or even to realise changes, yet his astonishment when he became convinced that a change was beneficial, and that he had not found it out before, was sometimes almost ludicrous. In politics he was an active Liberal, and took a prominent position in the party so far as concerned West Kent, and subsequently the Borough of Lewisham, until the recent "split," when he became a dissentient. As a friend, only those who knew him long and intimately can realise the loss they have sustained. As a benefactor, those who received proof of his generosity were many, but in this respect he "did good by stealth," and it was not until after his death that the writer had even

an idea of the pecuniary help received from him by some of his fellow-workers. In local affairs his interest and benevolence in the educational and charitable institutions of the parish of Lewisham are now, alas! matters of history.

For more than two years before his death his friends noticed with concern that his health was visibly and seriously impaired. A severe attack of eczema was followed by chorea, and this by severe gastric disturbance. About a year ago, indications of cancer of the stomach showed themselves, and became more and more confirmed. His illness was painless, but the great weakness constantly increased, as did his inability to retain or assimilate nourishment, and he passed peacefully away, in full possession of his mental faculties to the last.

In 1846 he married Isabel, the youngest daughter of Thomas Dunn, Esq., of Sheffield, but had no family. She survives him, and was his constant companion in all his work, travels, and excursions, and his devoted attendant during his long illness.

R. McL.

THOMAS ARCHER HIRST, the third and youngest son of Mr. Thomas Hirst, a wool-stapler, was born at Heckmondwike, in Yorkshire, on 22nd April, 1830. He was educated at the West Riding Proprietary School; and, in 1844 he became an articled pupil of a surveyor at Halifax. It was in this office that he made the acquaintance of John Tyndall, who became a life-long friend of Hirst, and exercised a deep influence on his scientific career; and, in particular, it was the example of Tyndall which led him to give up the pursuit of the profession at first chosen for him. Tyndall had left England to study chemistry, under Bunsen, at Marburg; and thither Hirst followed him, in 1849, to study mathematics, physics, and chemistry. After three years at that University he obtained the degree of Ph.D., on examination in his three subjects and an approved dissertation in analytical geometry. Subsequently, a short time was spent in Göttingen with Gauss and Weber, and then he went to Berlin where he attended lectures by Dirichlet, Steiner, and Joachimsthal. His intercourse with Steiner did much to determine the ultimate bent of his mathematical investigations; but some years elapsed before it was fully indicated, as the majority of his earlier papers are devoted to researches in mathematical physics.

He succeeded Tyndall, at Queenwood, in 1853, on the appointment of the latter at the Royal Institution; but this post was resigned in 1856 on account of the delicate health of his wife, whom he had married in 1854. The succeeding winter was spent at Biarritz and Pau, but without permanent good results, as Mrs. Hirst died in Paris, in 1857. Tyndall took him to Switzerland for six weeks,

and, on their return, left him in Paris, where the next winter was occupied in attending lectures by Chasles, Lamé, and others, and in making the acquaintance of the leading French mathematicians. In the winter of 1858–59 he was in Rome; and subsequently he travelled in Italy, making the acquaintance of many mathematicians, especially of his friend Cremona.

Returning to England in 1860, he was appointed mathematical master at University College School; and he held this post for a period of five years, during which he made his first developments in teaching geometry. The experience of the results then attained led him to join the Association for the Improvement of Geometrical Teaching, when it was formed in 1871; and for the first seven years of its existence he was its President and took an active part in its work.

He had been elected a Fellow of the Royal Society in 1861, and it was from this date onwards that his researches are devoted to the various branches of pure geometry which proved of most absorbing interest to him. He was appointed Professor of Physics at University College in 1865, and, on the death of Professor De Morgan in 1867, he succeeded to the Professorship of Pure Mathematics; but the latter chair he resigned in 1870 to become Assistant Registrar in the University of London. In 1873, the date of the establishment of the Royal Naval College at Greenwich, he was appointed Director of Naval Studies; and he continued to discharge the duties of that office for ten years. The precarious condition of his health then compelled him to resign: and he subsequently lived in comparative retirement, spending most of his winters abroad, until his death on 16th February, 1892.

Hirst took a prominent part in the foundation of the London Mathematical Society in 1865, served as its President from 1872 to 1874, and was a member of its Council for over twenty years. His active co-operation with the Society did much to extend its influence: and it was largely to the pages of its Proceedings that his papers on pure geometry were contributed. These are the papers containing the particular researches for which a Royal Medal was awarded to him in 1883—the year which practically marks the termination of his public life. He had served on the Council of the Royal Society in the years 1864–66, 1871–73, 1880–82: after 1883, the only sign of activity was the production of several papers, one of which in continuation of his earlier researches is of considerable importance.

The amount of Hirst's published work is not great; but his work is valuable, and an appreciable portion of it has been translated into French and into Italian. His papers are singularly clear: and side issues, that might lead him away from the main line of development of his subjects, are severely excluded. It is evident that he

bestowed great care not merely in carrying out his investigations, but in considering the form in which they are expressed: and his reluctance to premature publication of incomplete work may be gathered from the fact that, though, on quitting the Presidential Chair of the London Mathematical Society on 12th November, 1874, he made a brief statement of some results which he had obtained in the theory of Correlation of Space, the full exposition of his results was not communicated to the Society until 9th January, 1890.

The work with which his name as a mathematician will be most definitely associated is contained in his papers on the Correlation of Planes and the Correlation of Space. The simplest space of two dimensions is a plane, and the elements of such a space are a point and a straight line; a correspondence is established between two planes when all the elements of one plane are connected by a relation or relations with all the elements of the other. When the relations are such that, in general, one element of one plane is associated with one (and with only one) element of the other plane, and *vice versâ*, the correspondence is unique. If each point in one plane corresponds uniquely with a point in the other, and likewise each line in one plane uniquely with a line in the other, the correspondence is called a homography: the theory of homography is considered at length by Chasles in his 'Traité de Géométrie Supérieure.' If each point in one plane correspond uniquely with a line in the other, and likewise each line in the one plane with a point in the other, the correspondence is a correlation. A few properties of correlative planes are proved by Chasles in the treatise quoted: it is Hirst's distinction to have constructed the theory of correlation of planes and to have developed it to a great degree of perfection. The extension of the theory of correlation so that it may be applied to space of three dimensions was adverted to by Chasles in his 'Aperçu Historique'; the full extension was carried out by Hirst, whose investigations in this subject, together with those of his friends Rudolf Sturm, Cremona, and others, have resulted in important and substantial additions to the theory of pure geometry.

The following memoranda of Dr. Hirst are due to Professor Tyndall; the present state of his health is sufficient to account for their brevity.

A. R. F.

[*Memoranda concerning Dr. Hirst.*]

The "railway mania" was at its height, and profitable employment was in prospect for young men trained to the use of the theodolite, spirit-level, chain, and drawing-pen. Youths of well-to-do families were articled in numbers to a profession offering so many

attractions. Under such circumstances, Thomas Archer Hirst was articled, in 1846, to Mr. Richard Carter, then resident in Halifax, in whose office, at the time, I happened to be principal assistant. He was then about sixteen years of age. His father had been engaged in the wool trade, his mother was a widow, and he was the youngest of a family of three sons.

The West Riding of Yorkshire was then the battle-field of two great rival railway companies—the West Riding Junction and the West Riding Union. Our duty at the time consisted in the preparation of plans and sections of the lines proposed by the latter company. Save in the office, I saw but little of Hirst at the commencement—he being told off in the field to a party different from mine. But an intimacy gradually grew up between us, and in due time, though I was ten years his senior, we became steadfast friends. Influenced by the writings of Carlyle, Emerson, Fichte, and other philosophers, I held, in those days, very serious views of human conduct and duty. After some time, I noticed that my conversations with Hirst were producing a similar shade of earnestness in his mind. In 1848, I quitted England for Germany, choosing the University of Marburg, in Hesse Cassel, where, in regard to science, Bunsen was then the leading star. Hirst, instead of pursuing the profession chosen for him, soon resolved to follow me to that University. He paid me a preliminary visit in the summer of 1849. It was associated with a pathetic incident. Prior to the examination for the Doctor's Degree, it was customary for the candidates to visit the Professors, and to invite them to be present at the examination. I was on my way to the rooms of one of the Professors, when the postman, meeting me in the street, placed a letter in my hands. It was from a young colleague of Hirst's, who worked in the same office with him in Halifax. I was stunned by the perusal of its first few lines. Hirst had left his mother in good health, and this letter informed me of her sudden death. The writer told me that he had also written to Hirst, but that knowing his strong attachment to his mother, he was afraid to let him know the worst. He trusted to my discretion to disclose it to him in the gentlest manner possible. On returning to our lodgings, I found that Hirst had received the letter announcing his mother's illness, and was making preparations for his immediate return to England. He was far from well. In those days he frequently suffered from a malady of the throat, which entirely quitted him in later years. Everything being prepared for the journey, he had his trunk taken to the coach office, where, after waiting some time, he entered the *Post Wagen*. I was in great perplexity; for, while shrinking from imparting to him the knowledge in my possession, I could not bear to allow him to return, cherishing the delusion that his mother still lived. On squeezing his hand for

the last time, I said to him, "Dear Tom, prepare your mind for the worst." His reply was a startled, steadfast look, and, in a moment, he added, "John, my mother is dead. Tell me all; I can bear it." "Yes," I replied, "she is dead," and he drove away.

After my severance from Halifax, Hirst was accustomed to hold a little weekly symposium in his lodgings. A group of young fellows desiring intellectual intercourse used thus to meet, mainly for the discussion of questions touching upon religion. One of them, I remember, was the son of a Congregationalist minister; another a young author of considerable ability—at that time an ardent admirer of Carlyle, but who afterwards became an equally ardent Roman Catholic. Hirst had now grown into a tall man, with a singularly noble countenance. It was interesting, indeed, to observe how this nobility of expression increased as thought and aspiration mingled more and more with his physical conformation. His hair was dark, his forehead finely formed, his nose and general features well chiselled. The only exception that could be taken to the beauty of his countenance was a certain looseness of lip, which seemed to indicate a lack of firmness of character. But the indication was deceptive, for Hirst could be immovable when circumstances called forth the exercise of firmness.

There was, at that time, near Halifax, a tract of heath-land called Skircoat Moor, at one corner of which stood a little cottage called "The Birdcage." The widow who occupied this cottage eked out a livelihood by selling sweets to the children who came to the moor. She had one son, who had begun life as an errand boy in a printer's office, but who, by good conduct and intelligence, had risen to a highly respectable position in one of the mills. This youth, whose name was Booth, attended Hirst's symposium. His health began to fail, and Hirst observed with anxiety the increasing pallor of his countenance as he walked to and from his work. Medical advice was resorted to, and Booth's malady was pronounced to be consumption. His weakness increased, his usefulness as a clerk diminished, until at last Hirst insisted that he must cease working and direct the whole of his attention to the care of his health. As to his salary, he (Hirst) undertook to make that good. The poor youth lingered long. Hirst had followed me to Marburg, had quitted that University, and had become a favourite pupil of the illustrious Steiner at Berlin. The *Semester* had begun; its busiest time had set in. One morning, however, he made his appearance in London, and told me that Booth, who was obviously dying, had written, imploring his benefactor to visit him. In response to that letter, Hirst had quitted his studies and had come over to England. Travelling down to Halifax, he found that Booth's chief anxiety related to his mother. "What will become of her," he exclaimed, "when I am gone?" The means at

Hirst's command at the time were very moderate. Through the accidents of trade, or the misconduct of individuals, the property left by his father had, in great part, disappeared. Still, without a moment's hesitation, he gave the dying youth the comforting assurance that his mother would be properly taken care of. For some years afterwards, while Hirst continued in Germany, I was myself the intermediary through whom the widow's allowance passed to her hands.

The sum paid when Hirst was articled to Mr. Carter, together with a professional education of five years, was naturally, on the part of his relatives, expected to produce some tangible result; and when they found that he had resolved to relinquish it all for the sake of the cultivation of his intellect, they thought his resolution a wild one. But he never wavered for a moment, and, except on occasions when his health caused me anxiety, I never wavered in the conviction that the step he had taken was a wise one. Throughout the winter and spring in Marburg, our days were spent in labour, attending lectures, working in the chemical laboratory, and studying at home. At 10 P.M., the stroke of a piano by Hirst gave notice that the work of the day was ended. We had music and light reading afterwards, the latter including the 'Essays of Montaigne,' which proved to us a source of strength as well as of delight. At 11 P.M. we went to bed. I was earliest up, for, soon observing that hard study was telling upon the younger and less vigorous constitution, I persuaded him to give more time to repose.

After I had quitted Marburg, leaving Hirst behind me, Dr. Simpson, a medical man with a passion for chemistry, afterwards Professor of Chemistry at Queen's College, Cork, came to the University to pursue his studies in Bunsen's laboratory. He brought with him his wife and family and a sister of his wife. They were both sisters of John Martin, the pure-minded Young Irelander. Acquaintance ripened into friendship between the young people, friendship into love, and, after Hirst had taken my place at Queenwood College in 1853, he married Miss Anna Martin. A few years of unalloyed happiness were wound up by an attack of tuberculosis, which ended in her death in 1857. He had taken her to Biarritz, thence to Pau, whence he had returned to Paris, where she died. I was on my way to Switzerland when intelligence of the calamity reached me. Ignorant of their address in Paris, I sought him in his old qnarters. Failing to find him there, and guided by a vague indication, I sought him in the Rue Marbœuf. Here, though baulked at first, I discovered where he lodged. On turning, afterwards, the corner of a street, I met him face to face, looking as white as marble. He was returning from ordering his wife's coffin.

I stood beside him in the cemetery of Père la Chaise when she was

lowered into the grave, and afterwards carried him with me to the Alps. Prior to joining me at Chamounix, he had three days of lonely wandering; communing with himself under his changed conditions. We made the little *auberge* at the Montanvert, which was then a very small affair, our permanent residence. For six weeks his life was filled with healthy exercise, under conditions where constant attention was necessary to his personal safety. Nothing could have been better calculated to divert his mind from the grief which weighed upon it. A few days after our arrival, we were joined by Professor Huxley. As we assembled night after night round our pine-wood fire, life became to all of us more and more a thing to be enjoyed. We returned from the Alps, Hirst halting in Paris, where for some time he took up his abode. Here he made the acquaintance and secured the friendship of the foremost mathematicians. He went afterwards to Italy; and was at the village of Solferino, helping the wounded, on the day after the battle. In Italy he met Professor Cremona, who remained his friend in a very special sense to the end of his life. On his return to London, Hirst became mathematical master in University College School, then Professor of Applied Mathematics in University College. For the sake of his health, he afterwards accepted the Assistant Registrarship of the University. His final appointment was to the Post of Director of Studies in the Royal Naval College, Greenwich, under the presidency of Admiral Sir Cooper Key, who was succeeded by Admiral Fanshawe; Mr. Goschen was at the time of his appointment First Lord of the Admiralty. Hirst never forgot the Minister's high-minded sympathy with his mathematical studies, or his willingness so to arrange matters as to reconcile the prosecution of those studies with his duties as Director.

During his later years, the state of Hirst's health frequently caused his friends the gravest anxiety. He relinquished in succession the posts he had occupied, retiring finally from Greenwich with a Government pension. During the present calamitous year, he was smitten with influenza, to which he finally succumbed.

I have already given an example of Hirst's kindness of heart. To this it may be added that, apart from his scientific labours, his life throughout was one of wise beneficence.

J. T.

Edward Killwick Calver, born 1813, entered the Royal Navy in 1828, in the navigating branch, on board H.M.S. "Crocodile." After a further term of foreign service in the "Satellite," he in 1836 commenced his long period of service as a marine surveyor on the Home Station. For thirty-six years, with scarcely a break, he served in this capacity mainly on the south and east coasts of Great Britain.

From 1847 to 1871 he was in charge of this survey, and produced charts of nearly the whole of the eastern coasts.

Captain Calver's constant employment in the rivers and estuaries of the east coast gave him great experience in the action of tides, waves, and running water; and he paid much attention to studying the principles which should underlie works for the conservation of depth and improvements of tidal harbours and entrances. His views on this subject were remarkably sound. They were set forth in various pamphlets, the principal work being entitled, "On the Conservation and Improvement of Tidal Rivers," published in 1853. In this he anticipated many of the principles which have now for long been accepted in carrying out artificial works; and his predictions as to the results of various artificial works have nearly all been verified. He made several valuable reports to the Admiralty on the subject of harbours of refuge.

Captain Calver retired in 1871, and died at Vevey, in Switzerland, where he had of late resided, on 28th October, 1892.

Captain Calver became an Associate of the Institution of Civil Engineers in 1866, and was elected a Fellow of the Royal Society in 1873.

W. J. L. W.

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